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ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

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NATURAL HISTORY,

AGRICULTURE, AND THE ARTS.

BY THOMAS THOMSON, M.D. F.R.S. L. & E. F.L.S. &c.

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IMPERIAL MEDICO-CHIRURGICAL ACADEMY OF PETERSBURGH.

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Page 222, line 9, *for* Cephaloferes, *read* Cephalophores.
—— 223, —— 9, — echidra, *read* echidna.
—— 223, —— 16, — raphores, *read* raptores.
—— 223, —— 16, — scausores, *read* scansores.
—— 223, —— 43, — on, *read* in.
—— 224, —— 11, — Cephalopheres, *read* Cephalophores.
—— 224, —— 14, *after* classes, *add* on.
—— 224, —— 38, *for* entomoertracea, *read* entomostraca.
—— 225, —— 13, — siparculi, *read* sipunculi.
—— 225, —— 19, — Polypaeres, *read* Polypiaires.
—— 225, —— 27, 28, 29, *for* infusaria, *read* infusoria.
—— 226, —— 15, *for* of, *read* in.
—— 226, —— 28, — of, *read* on.
—— 228, —— 7, — swell, *read* smell.
—— 228, —— 13, — (Lutr.) *read* (Latr.)

ANNALS

OF

PHILOSOPHY.

JANUARY, 1817.

ARTICLE I.

Account of the Improvements in Physical Science during the Year 1816. By Thomas Thomson, M.D. F.R.S.

I HAVE endeavoured since the commencement of the *Annals of Philosophy* to lay before the British public every chemical fact that has come to my knowledge, in whatever part of the world the discovery took place. But the great number of original communications, which nearly fill this Journal, preclude the possibility of translating the papers published on the continent in such numbers as I originally intended. The object of the historical sketch, with which each new year of the *Annals* commences, is to supply the place of this omission. It costs me considerable trouble; but it saves a great deal to the reader, by giving him in a small compass what, when originally published, occupied many volumes. The chemical and mineralogical department of this sketch will always be found the most complete. I compose them first, and endeavour to leave nothing out. The extent to which I can carry the other sciences depends upon the room still left after these are completed. On the present occasion I find myself so much at a loss for room, that I have thought it right to omit every thing contained in the Philosophical Transactions, and in the Transactions of the Geological Society, for 1816. These omissions I propose to supply afterwards, by giving an analysis of the contents of those volumes.

I. MATHEMATICS.

Perhaps in strict propriety I ought to omit the mention of mathematical papers; but as the list of them which I give occupies but little room, and as we have no British publication appropriated to the subject, I conceive it better to notice them.

1. In the *Annals of Philosophy* for 1816 some mathematical papers of considerable importance have appeared. I consider the anonymous paper on the Present State of the Mathematical Sciences in Great Britain (*Annals*, vii. 89) as a very just and fair picture, and hope it will have the effect of rousing the latent energies of our countrymen, and of producing another mathematical era in this island not inferior in brilliancy to that in which Wallis, Barrow, Newton, Cotes, Maclaurin, and many other illustrious mathematicians, made their appearance. As mathematics require encouragement more than any of the other sciences, there can be no doubt that its cultivation would be greatly promoted if a society could be formed to facilitate the publication of mathematical papers by defraying the requisite expense. I believe that such a society might be established without difficulty, if any person of sufficient influence could be found to patronize it.

2. The paper on the Quadrature of the Circle (*Annals*, viii. 13) is likewise curious and interesting.

3. The other mathematical papers in the *Annals* are—A Demonstration that the Ellipse in certain Positions appears circular (*Annals*, vii. 205): A general Demonstration of the Binomial Theorem (Ibid. vii. 346): A Demonstration of a curious Relation between the various Orders of Differences, by Mr. Harvey (Ibid. vii. 475): Theorems for determining the Amount of Annuities increasing in the constant Ratio of the natural Numbers $1 . 2 . 3 \ldots . n$, by Mr. Benwell (Ibid. viii. 119): On Annuities, Imaginary Cube Roots, and Roots of Binomials, by Mr. Horner (Ibid. viii. 279): and the Solution of a curious Mathematical Problem, by Mr. Ivory (Ibid. viii. 272).

4. In the first number of the Journal of the Royal Institution (p. 6) there is a demonstration of a considerable number of Dr. Stewart's Problems, by Mr. Babbage.

5. On the Developement of Exponential Functions, together with several new Theorems relating to Finite Differences, by J. F. W. Herschel, Esq. F.R.S. (Phil. Trans. 1816, p. 25.)

6. An Essay towards the Calculus of Functions, Part II., by C. Babbage, Esq. F.R.S. (Phil. Trans. 1816, p. 179.)

7. A new Demonstration of the Binomial Theorem, by T. Knight, Esq. (Phil. Trans. 1816, p. 331.)

8. On the Fluents of Irrational Functions, by E. F. Bromhead, Esq. (Phil. Trans. 1816, p. 335.)

II. ACOUSTICS.

1. *Influence of the Wind on the Propagation of Sound.*—In the Ann. de Chim. et Phys. i. 176, there is a curious set of experiments on this subject by M. Delaroche. His method was to have two drums or bells giving exactly the same sound. The experi-

menter was placed between them; and he varied his position till both the sounds appeared the same. The distance between him and each of the sounding objects was then measured. Some of the results of these experiments are rather paradoxical, or at least they contradict the commonly received opinions. They are as follows:—

(1.) The wind has scarcely any influence on sounds at small distances; 20 feet, for example.

(2.) When the distance is more considerable, the sound extends much less against the wind than in the direction of the wind. The difference increases with the distance.

From these two propositions it results, 1. That the law of the decrement of sound is not the same in the direction of the wind and in the opposite direction. 2. That the influence of the wind upon sound is not greater at the place where the sound is produced than it is during the whole course of its passage.

(3.) Sound is heard a little better in a direction perpendicular to the wind than in the direction of the wind itself.

(4.) Causes not connected with the wind, but depending upon the modifications of the atmosphere, have great influence on the facility with which sound is propagated to a distance.

III. OPTICS.

1. *Refractive and Dispersive Powers of certain Bodies in the State of Liquid and Vapour.*—MM. Arago and Petit have made an important set of experiments on the refractive power of certain bodies while in a liquid state, and afterwards upon the same bodies when converted into vapour. Supposing the Newtonian theory to be correct, it is natural to conclude that the refractive power of the same body in different states is always as its density. But from the experiments it appears that when a body is converted into vapour its refractive power diminishes at a greater rate than its density. Thus the refractive power of liquid carburet of sulphur, when referred to air, is a little greater than 3; but when referred to air in the state of vapour, its refractive power is only 2. The substances experimented upon were carburet of sulphur, sulphuric ether, and muriatic ether. The dispersive power of these bodies when they are converted into vapour diminishes at a greater rate than their refractive power. (Ann. de Chim. et Phys. i. 1.)

2. *Remarkable Phenomenon observable in the Diffraction of Light.*—When an opaque body is placed in a pencil of light, its shade is surrounded externally by certain bands of light which are particularly examined by Newton in the third book of his Optics. Luminous bands not less remarkable appear likewise within the shadow. These were described by Grimaldi, and afterwards by Maraldi and Delisle. In the year 1803 Dr. Thomas Young published in the Phil. Trans. a very curious experiment respecting these internal bands. They disappear entirely provided the rays that pass along either of the sides of the opaque body be stopped by an

opaque screen. Arago has observed that a piece of glass plate of a certain thickness may be substituted for the opaque screen with a similar effect. If the glass be very thin, the bands do not disappear, but are displaced from their former situation. This displacement increases with the thickness of the glass, till at last the bands disappear entirely. (Ibid. i. 199.)

3. *Diffraction.*—There is an important paper on this subject by M. Fresnel in the Ann. de Chim. et Phys. i. 239, in which he gives an account of a curious set of observations, and gives a theory of diffraction founded on the hypothesis that light is owing to the undulations of a subtile fluid. It is not possible here to give an idea of his reasoning without entering into details which would be incompatible with the limited extent of this sketch; but the paper deserves the attention of all those who are interested in the science of optics.

IV. STATICS.

1. Upon this subject the most important set of experiments published during the course of the year is to be found in Col. Beaufoy's paper on the Stability of Vessels, published in the *Annals of Philosophy*, vii. 184. This paper is of such a nature as to preclude the possibility of giving an abstract of it. I must, therefore, rest satisfied with referring the reader to the paper itself. These experiments agree with the doctrine laid down by Mr. Richard Hall Gower in his Observations on the present Construction of Ships, a book printed in 1807; but as almost the whole impression was consumed by the burning of Mr. Bensley's house, Bolt-court, Fleet-street, on Nov. 5, 1807, it has probably fallen into the hands of a very small number of readers.

2. I may here also mention Col. Beaufoy's important paper on the Resistance of Air, and on Air as a moving Power, printed in the *Annals of Philosophy*, viii. 94, though it belongs rather to pneumatics than statics. It is also incapable of abridgment, but deserves the particular attention both of philosophers and practical engineers.

3. To Col. Beaufoy we are indebted, likewise, for an ingenious contrivance for determining an invariable standard of measure by the distance which a ball falls in a given time. His description is so concise that I must here also refer the reader to the original paper in the *Annals of Philosophy*, viii. 211.

V. ELECTRICITY.

1. *Zamboni's Column.*—A great number of papers have been published on this new electrical instrument; but no new fact of any importance has been brought to light. Dr. Schübler, of Hofwyl, has shown that it has no connection with the electrical state of the atmosphere (Schweigger's Journal, xv. 111, 126; xvi. 111). Heinrich found, as had been already observed by others, that the motion of the pendulum varies considerably in its velocity. On Nov. 10,

1815, it vibrated 500 times in 4′ 32″; while on Oct. 3 of the same year it took 10′ 5″ to make the same number of vibrations. During the month of Sept. 500 vibrations usually occupied between 7′ and 8′; during Oct. between 4′ and 6′ (Schweigger's Journal, xv. 113). Schweigger and Grindel have shown that Zamboni's column always contains moisture when in a state of activity (Ibid. xv. 132, 479). Iäger, of Stuttgard, has published an elaborate theory of this column (Gilbert's Annalen, lii. 81), and important remarks on the same subject have appeared by Professor Pfaff, of Kiel (Ibid. lii. 108). But as these papers, though well drawn up, do not appear to me to contain any thing essentially different from the explanations already given in this country, I do not consider it as necessary to enter into details respecting them. It is needless to notice the clocks that have been constructed by means of this column as a moving power both in this country and in Germany; because it is obvious that the great irregularity in the motion of these pendulums must render such clocks of no real utility.

From a paper on Zamboni's column by Gay-Lussac (Ann. de Chim. et Phys. ii. 76), we learn that the first attempt to construct a dry galvanic column was made by Desormes and Hachette in 1803. But it would appear from his account that this attempt was not attended with success. Deluc was in reality the first successful constructor of this column. His curious memoir was read to the Royal Society in 1809, and published in Nicholson's Journal in 1810. Zamboni's column was first constructed in 1812, and described by him in a memoir published in Verona, entitled, Della Pila Electrica a Secco, &c. I gave an account of it in the historical sketch for the preceding year.

2. In the year 1814, Confiliacchi, who succeeded Volta as Professor of Natural Philosophy at Pavia, published a treatise on the Identity of Electricity and Galvanism. This treatise was written by an anonymous friend and pupil of Volta, who had drawn it up, but had been prevented from publishing it by a premature death. It professes to give merely the theory and views of Volta. To judge from the account of it given in the Bibliotheque Britannique, (xxxviii. 305) and by Professor Gilbert (Annalen, li. 341), for I have not seen the original work, it seems to constitute the most complete treatise on galvanism which has hitherto appeared. The paper of Dr. Weber (Gilbert's Annalen, li. 353) likewise deserves attention in a theoretical point of view. It contains a pretty full and clear exposition of the phenomena, arranged with the view of elucidating the theory of this obscure branch of electricity.

3. *Metallic Hydrurets.*—It has been supposed that when different metallic wires are employed to complete the circuit of a galvanic battery by proceeding from the minus pole into a vessel of water, these metals combine with the hydrogen of the decomposed water, and form hydrurets. Several of these hydrurets have been long ago described by Ritter and by Brugnatelli. In Schweigger's Journal, xv. 411, there is a paper by Ruhland, in which he describes a con-

siderable number of these hydrurets. I forbear to give the details, because I do not perceive sufficient proofs that the substances described are hydrurets.

4. In the Ann. de Chim. et Phys. ii. 59, there is a curious memoir by M. Dessaignes on the influence of *temperature* on electricity. He shows that heat and cold both alter the quantity and the kind of electricity. The facts are important, because they bring into view a new branch of electricity, which has hitherto been neglected. But they are of such a nature as not to be susceptible of abridgment. I shall, therefore, lay a translation of them before my readers in a future number of the *Annals*.

5. In the *Annals*, viii. 74, there are two very curious galvanic experiments by Mr. Porret, which deserve the particular attention of physiologists and electricians. He found that when a galvanic battery has become inefficacious, it again recovers its energy by withdrawing the greater part of the liquid from the cells so as to uncover the plates of metal. He found that galvanic electricity has the property of forcing water through the coats of a bladder, so as to cause it to accumulate on the negative side in a vessel of water divided into two by a perpendicular diaphragm of bladder.

VI. CHEMISTRY.

This science, as usual, will occupy the greatest portion of our historical retrospect. We shall follow our former method of subdividing the facts which we have to detail, and of placing them under various heads, for the convenience of the reader.

I. APPARATUS.

Chemical experimenting has of late years been brought to a much greater precision than was formerly thought possible; owing to the gradual improvement and simplification of the apparatus employed. Every amelioration, therefore, in any chemical instrument whatever, deserves the attention of the practical chemist. On this account I think it worth while to notice some improvements that have been made known in London during the course of the preceding year.

1. Mr. Newman, of Lisle-street, so well known to chemists as an ingenious maker and improver of chemical instruments, has proposed the following mercurio-pneumatic apparatus, which promises to be of considerable utility when experiments are to be made upon the gases rapidly absorbed by water, and which require, in consequence, to be confined over mercury. It consists in combining Mr. Pepys's gasometer with the common mercurial trough. The engraving (Plate LX. Fig. 1) exhibits an outline of the apparatus as represented by Mr. Newman in the Journal of the Royal Institution, i. 185.

It requires about 70 lb. of mercury to fill it. The trough has a cavity in the middle large enough to fill a jar 10 inches long, and 2¼ wide; and there is a shelf on each side, three inches in width, to support vessels containing gas. Opposite to three indentations on the edge of the trough are three holes in one of the shelves, into which the beak of retorts liberating gas are to be introduced, or a sliding shelf and apertures may be fitted across the cavity for the same purpose. The gazometer is at one end, and sunk below the level of the trough. It is capable of containing 50 cubic inches. A tube connected with the gazometer at the lower part is made to ascend, and passing up through the mercury in a corner of the trough, at about an inch above, it bends down again, and terminates between its surface. If gas is contained in the gazometer, it may be transferred to air jars in the trough, by filling them with mercury, placing them over the end of this bent tube, and giving pressure to the gazometer. The air will pass from the gazometer along the tube into the jar. By the bend in the tube, the mercury is prevented from passing into the lower part of the gazometer, while at the same time the gas is allowed a free passage. All inconvenience is prevented by means of a stop-cock, which shuts off the communication between the receiver and the trough, preventing as the same time the escape of air from the gazometer, and of mercury into it. A sliding shelf is fixed beneath the trough to support a spirit lamp under a retort, or for other purposes. A detonating tube and spring are also attached to the apparatus by a clamp and screws, and may be fixed on any side of the trough. The whole apparatus is of iron, excepting sometimes the pillars which support it, and which may be of brass. It is not more than 18 inches in length and height. It is placed in a large japanned tray to collect scattered mercury.

2. Mineralogists are indebted to Mr. Brooke for a very ingenious and valuable improvement of the common blow-pipe. A description of it by Mr. Brooke himself will be found in the *Annals*, vii. 367. It consists of a close box, into which air is condensed by means of a syringe. From this box the air is allowed to rush upon the flame of a lamp or candle, and thus produces all the effect of the common blow-pipe, while both the hands and mouth are left disengaged.

By means of this new blow-pipe filled with a mixture of two volumes hydrogen and one volume oxygen gas, some very curious and important experiments have been made by Dr. Clarke, an account of which he has published, partly in the Journal of the Royal Institution, iii. 104, and partly in the *Annals*, viii. 357. He found the heat produced in this way capable of fusing all substances tried, excepting only charcoal and plumbago. All the most refractory stones, the earths, namely, lime, barytes, strontian, magnesia, alumina, and silica, were melted into glass, slag, or enamel. But the most unexpected result was the reduction of barytes and strontian into their metallic bases. Of these metals thus obtained I have seen specimens. They were white, had a silvery lustre, and a specific gravity exceeding 4. But they were not in a state of perfect purity, being mixed with slag and with unreduced earth. To these metals Dr. Clarke has given the names of plutonium and strontium.

II. NEW CLASSIFICATION OF CHEMICAL BODIES.

The recent discoveries in chemistry have occasioned a very considerable revolution in the theory of that science. Whoever has paid sufficient attention to these improvements must be sensible that the present arrangement of chemical bodies is in many respects imperfect and inconvenient. The undecomposed bodies at present known amount to about 48, all of which, except eight, are considered as metals. I had occasion to touch upon this subject some months ago in my review of Professor Jameson's Mineralogy (*Annals*, viii. 136). and pointed out one or two alterations which appeared to me necessary. About the same time an elaborate dissertation on this subject by M. Ampere appeared in the Ann. de Chim. et de Phys. (i. 295, 373; ii. 5, 105). He examines the properties of all the simple bodies in detail, with much acuteness and discrimination, and endeavours to form them into a natural system, in which they follow each other according to their properties. I have not room at present to examine this arrangement with the minuteness which would be requisite in order to determine its accuracy, or to point out the reasons which induce me to dissent from some of his conclusions. I shall satisfy myself with giving the following outline of the classification.

The simple substances naturally subdivide themselves into three classes, namely,

1. Gazolytes, or substances capable of forming permanent gases with each other.
2. Leucolytes, or metals fusible below 25° Wedgewood, and whose oxides form colourless solutions with the colourless acids.
3. Chroicolytes, or metals requiring a higher temperature for fusion than 25°, and whose oxides form coloured solutions in colourless acids.

Class I. Gazolytes.

Genus 1. BORIDES. (From *boron.*)

Bodies forming permanent Acid Gases with Phthore.[*]

Sp. 1. Silicon. Sp. 2. Boron.

Genus 2. ANTHRACIDES. (From *ανθραξ*.)

Bodies combining with one of the Elements of Air when exposed to it at a sufficient Temperature, and forming permanent Gases with the other Element.

Sp. 1. Carbon. Sp. 2. Hydrogen.

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[*] *Phthore* is the name by which M. Ampere has thought proper to distinguish the hypothetical body called *fluorine* by Sir H. Davy.

Genus 3. THIONIDES. (From θειον.)

Bodies capable of uniting with the preceding Genus, and of forming gaseous or very volatile Compounds.

Sp. 1. Azote.
2. Oxygen.
Sp. 3. Sulphur.

Genus 4. CHLORIDES. (From *chlorine.*)

Bodies unalterable in the Air at all Temperatures, forming with Hydrogen Acid Compounds gaseous or very volatile.

Sp. 1. Chlorine.
2. Phthorine.
Sp. 3. Iodine.

Genus 5. ARSENIDES. (From *arsenic.*)

Bodies oxidated in the Air when exposed to it at a sufficient Temperature, forming solid Compounds with Oxygen, and permanent Gases with Hydrogen.

Sp. 1. Tellurium.
2. Phosphorus.
Sp. 3. Arsenic.

CLASS II. LEUCOLYTES.

Genus 1. CASSITERIDES. (From κασσιτερος.)

Bodies whose Combinations with Oxygen are decomposed by Carbon, but not by Iodine.

Sp. 1. Antimony.
2. Tin.
Sp. 3. Zinc.

Genus 2. ARGYRIDES. (From αργυρος.)

Bodies whose Oxides are decomposed by Iodine and Hydrogen.

Sp. 1. Bismuth.
2. Mercury.
Sp. 3. Silver.
4. Lead.

Genus 3. TEPHRALIDES. (From τεφρας and ἁλς.)

Bodies whose Oxides are decomposed by Iodine, and not by Hydrogen.

Sp. 1. Sodium.
Sp. 2. Potassium.

Genus 4. CALCIDES. (From *calcium.*)

Bodies whose Oxides are not decomposed by Carbon or Iodine, but by Chlorine.

Sp. 1. Barium.
2. Strontium.
Sp. 3. Calcium.
4. Magnesium.

Genus 5. ZIRCONIDES. (From *zirconium.*)

Bodies whose Oxides are not decomposed by Chlorine, Iodine, or Carbon.

Sp. 1. Yttrium.
2. Glucinium.
Sp. 3. Aluminium.
4. Zirconium.

CLASS III. CHROICOLYTES.

Genus 1. CERIDES. (From *cerium.*)

Bodies brittle and infusible at the Temperature at which Iron melts.

Sp. 1. Cerium.
Sp. 2. Manganese.

Genus 2. SIDERIDES. (From σιδηρος.)

Bodies whose Oxides dissolve in Acids in a State of Purity, and form coloured Solutions only when concentrated, and whose Peroxides have not Acid Properties.

Sp. 1. Uranium.
2. Cobalt.
3. Iron.
Sp. 4. Nickel.
5. Copper.

Genus 3. CHRYSIDES. (From χρυσος.)

Metals unalterable in the Air at all Temperatures.

Sp. 1. Palladium.
2. Platinum.
3. Gold.
Sp. 4. Iridium.
5. Rhodium.

Genus 4. TITANIDES. (From *titanium.*)

Infusible Bodies whose pure Oxides do not dissolve in Acids, and do not form with the Alkalies Compounds which can be considered as true Salts.

Sp. 1. Osmium.
Sp. 2. Titanium.

Genus 5. CHROMIDES. (From *chromium.*)

Bodies infusible at the Temperature at which Iron melts, acidifiable by Oxygen.

Sp. 1. Tungsten.
2. Chromium.
Sp. 3. Molybdenum.
4. Columbium.

III. AFFINITY.

1. *Effect of Trituration on Chemical Combination.*—In the *Annals*, vii. 426, I have published a set of experiments by Mr. Link to determine what happens when dry salts, that mutually decompose each other when in solution, are triturated together. He found that when the two salts were destitute of water of crystalliza-

tion, no decomposition took place. But if either of them contained water of crystallization, they in that case mutually decomposed each other. When, after the trituration, a liquid capable of dissolving any of the constituents is poured on, decomposition takes place. It would appear from these experiments that the water of crystallization, though solid, still continues to exert its solvent powers.

2. *Structure of Solid Bodies.*—A very curious and important paper, by Mr. Daniell, has been published in the Journal of the Royal Institution (i. 24), which throws much new light upon the structure of solid bodies. If a lump of alum, or borax, or of nitre, be immersed in a vessel of water, and left at rest for three or four weeks, the solution will be found to have gone unequally on; the uppermost portion will be found most wasted, and the undermost least; so that the undissolved part of these salts will have assumed a conical form. The lower part of these bodies, after this treatment, will be found embossed over with numerous crystalline forms. These in alum are octahedrons, or figures formed by different sections of the aluminous octahedron. In borax they are fragments of eight-sided prisms, and so on. Mr. Daniell has shown in a satisfactory way that these embossments are not formed by the crystallization of that portion of the salt which has been dissolved; but that they are brought into view by the unequal solution of the lump of salt subjected to the action of the water. Hence it follows that all these apparently amorphous masses are in reality composed of crystals, though such a structure cannot be distinguished by the eye previous to this natural dissection of it. The same crystalline structure was developed when carbonate of lime, carbonate of strontian, and carbonate of barytes, were slowly acted on by vinegar. Bismuth, antimony, and nickel, treated with very dilute nitric acid, likewise exhibited a crystallized structure. From these experiments we may infer, with considerable probability, that the structure of most bodies is in reality crystallized, even when they appear amorphous. If this mode of natural dissection could be applied to minerals in general, it would greatly extend the Haüyan method, and remove most of the objections to which it is at present exposed.

Mr. Daniell terminates his paper by an ingenious examination of the structure of crystals, and shows that Dr. Wollaston's hypothesis, that the integrant particles of bodies are spherical or spheroidal, will alone agree accurately with all the phenomena.

3. *Anomaly in Affinity.*—Though chemists have long been aware that almost all the phenomena of their science depend upon what is called *affinity*, or upon the attractions which exist between the atoms of different bodies, no progress whatever has yet been made in measuring the intensity of these forces. It was early laid down as an axiom that bodies having an affinity for each other are attracted each by a specific force which varies according to the body, and that when two bodies, *a* and *b*, are united, if a third

body, *c*, be presented, which has a stronger affinity for *a* than *b* has, then *b* is completely disengaged, and *c* takes its place. This opinion was embraced by Bergman, and illustrated at considerable length in his Essay on Elective Attractions. Berthollet, without calling in question the truth of the axiom, denied that complete decomposition is ever produced, and affirmed that, if it take place, it must always be ascribed to a cause different from affinity, which is a power capable only of producing combination, and not decomposition. Though Berthollet has not perhaps succeeded in establishing his own hypothesis, I conceive him to have shown in a satisfactory manner that the previously received opinions were inaccurate, and that at present we have no very precise notions on the subject. A paper by Mr. Richard Phillips, published in the Journal of the Royal Institution (i. 80), sets this, if possible, in a still stronger light. It has been long known that carbonate of potash has the property of decomposing sulphate of barytes. Mr. Phillips was informed by Dr. Babington that carbonate of barytes may likewise be decomposed by sulphate of potash. Mr. Phillips's paper consists of an account of a set of experiments which he made to verify both these decompositions. 100 of sulphate of barytes being mixed with 59 of carbonate of potash, and a sufficient quantity of water, the mixture was boiled for two hours: 23 of the sulphate of barytes were decomposed. On the other hand, 85 parts of carbonate of barytes being mixed with 74 parts of sulphate of potash dissolved in water, after similar treatment, 57 parts of the carbonate of barytes were decomposed. It would appear from this, that six integrant particles of carbonate of potash are requisite to decompose one integrant particle of sulphate of barytes; while, on the other hand, three integrant particles of sulphate of potash decompose two of carbonate of barytes. It would seem from this that the united affinities of sulphuric acid for barytes, and of carbonic acid for potash, are in reality the strongest. The other decomposition appears to be the consequence of the great quantity of carbonate of potash present, and to cease when that quantity is diminished to a certain degree. If this explanation be well founded, there ought to be certain proportions of these two salts which would not act upon each other at all.

4. *Dalton's Theory of the Absorption of Gases by Liquids.*—In the year 1805 Mr. Dalton published a paper, entitled, On the Absorption of Gases by Water and other Liquids (Manchester Memoirs, Second Series, vol. i.) This paper contains a theory of the absorption of gases by liquids, and is remarkable for that ingenuity and simplicity which so peculiarly distinguish all Mr. Dalton's labours. His object is to prove that the absorption is altogether *mechanical*, and occasioned by the pressure of the atmospheres superincumbent on the liquids. To this theory two objections present themselves, which have hitherto appeared to me insurmountable. 1. Water absorbs many times its volume of certain gases.

In such cases there must exist a chemical affinity between the liquid and the gas absorbed. Yet in every other respect the absorption of these gases is similar to that of the least absorbable gases. Hence if we admit the action of chemical affinity in the one case, I do not see how we can refuse it in the other. 2. Water absorbs a determinate bulk of every gas. But the proportion absorbed varies exceedingly in the different gases. Of some gases it absorbs its own bulk; of others, only $\frac{1}{64}$ of its bulk. Now if the absorption be merely mechanical, I can see no reason for this difference; but if there be an affinity between the gas and the liquid, the quantity absorbed must depend upon the balance between the affinity and the elasticity of the gas, and of course must be regulated by that affinity.

In the year 1812 M. de Saussure published a very elaborate paper On the Absorption of Gases by different Bodies. This paper I translated, and inserted in the sixth volume of the *Annals*. The experiments relate chiefly to the absorption of gases by solid bodies. But the author added a section, in which he examines the absorption of gases by liquids. He shows, 1. That the law established by Dalton, and considered by him as a necessary consequence of the absorption of gases by liquids, being mechanical, does not hold; namely, that liquids absorb always $\frac{1}{1^3}$, $\frac{1}{2^3}$, $\frac{1}{3^3}$, or $\frac{1}{4^3}$, of their bulk of gases. 2. That the quantity of the same gas absorbed by different liquids is not the same, as Dalton had supposed, but very different. 3. That the order of the absorption of gases differs in different liquids. Thus naphtha absorbs more olefiant gas than it does of carbonic acid, while olive oil absorbs more carbonic acid than olefiant gas. Saussure also endeavoured to prove by experiment that the absorption of mixed gases by liquids does not follow the law established by Mr. Dalton. But Mr. Dalton in his vindication of his theory (*Annals*, vii. 215), has shown that Saussure's experiments coincided with his theory, provided we substitute the rate of absorption as determined by Saussure for what he himself had established. When I said "it would appear from these experiments of De Saussure that Mr. Dalton's theory is erroneous in every particular," I alluded to the theory as given by Mr. Dalton in his original paper. My observation could not be supposed to apply to any new modification of the theory founded upon these very experiments. I find myself still unable to assent to the opinion that the absorption of gases by liquids is entirely mechanical, because several of the phenomena appear to me incompatible with that opinion. Mr. Dalton will find, if he take the trouble to consult the third volume of the fourth edition of my System of Chemistry (p. 517), that I considered his experiments on the absorption of mixed gases by water as accurate, though I explained the fact without having recourse to the doctrine of mere mechanical absorption.

IV. CRYSTALLIZATION.

1. *Supposed Effect of Air on the Crystallization of Liquids.*—M. Geiger, of Heidelberg, relates a fact which he considers as illus-

trating the effect of air in producing the crystallization of liquids. 2½ oz. of acetic acid, obtained by distilling a mixture of acetate of potash and bisulphate of potash, were put into a four-ounce phial fitted with a ground stopper. This phial may be exposed to a temperature of 14° or 9·5° without freezing. But the instant the stopper is taken out, it congeals. Pretty brisk agitation does not occasion congelation. The crystallization is not impeded, though the phial be opened in a room of the temperature of 50°. (Schweigger's Journal, xv. 231.)

The cause of the crystallization of liquids under similar circumstances has not been completely developed. Sir Charles Blagden showed many years ago that it is not the air which occasions the change. The observations of Dr. Coxe on this subject (*Annals*, vi. 101) deserve attention.

2. *Crystallization of Lime.*—M. Gay-Lussac has hit upon a very ingenious method of crystallizing lime. He exposes lime-water in an exhausted receiver of an air-pump along with concentrated sulphuric acid. When the acid becomes weak, it is withdrawn, and new acid substituted in its place. The lime gradually crystallizes, and assumes the form of a six-sided prism. (Ann. de Chim. et Phys. i. 334.)

V. ATOMIC THEORY.

1. *Atoms of Iron, Zinc, and Manganese.*—Dobereiner has published a set of experiments on the oxides of these metals. (Schweigger's Journal, xiv. 206.) The black oxide of iron is composed of 100 metal + 30 oxygen, and the red oxide of 100 metal + 45 oxygen. The oxide of zinc is composed of 100 metal + 22·5 oxygen. These experiments would make the weights of the atoms of iron and zinc—

Iron	3·33 or 6·66
Zinc	4·44

I have already given the results of my experiments with these metals. I am disposed to consider the real weights of the atoms of these metals—

Iron	3·5 or 7·00
Zinc	4·0

From the experiments of Dobereiner, it appears that the black manganese ore of Transylvania is a sulphuret of manganese, and not a sulphureted oxide, as would result from the experiments of Klaproth and Vauquelin. He found it a compound of 100 metal + 52 sulphur. This would make an atom of manganese 3·84 or 7·7. The real weight is probably the same as that of iron.

2. *Azote.*—Dobereiner considers azote as an elementary substance. He thinks it capable of combining with four doses of oxygen in the following way:—

	Azote.	Oxygen.
1. Air, or protoxide of azote, composed of	1 atom	+ 1 atom
2. Nitrous oxide, or deutoxide of azote..	1	+ 2
3. Nitrous gas	1	+ 4
4. Nitric acid	1	+ 8

I noticed some months ago Gay-Lussac's new experiments on the compounds of azote and oxygen. His results are—

	Azote.	Oxygen.
1. Nitrous oxide, composed of	1 atom	+ 1 atom
2. Nitrous gas	1	+ 2
3. Pernitrous acid	1	+ 3
4. Nitrous acid	1	+ 4
5. Nitric acid	1	+ 5

What he calls *pernitrous acid* is the substance formerly distinguished by the name of *nitrous acid.* His *nitrous acid* is the nitrous vapour of former chemists. When I gave a sketch of Gay-Lussac's paper in the *Annals*, (viii. 71), I stated that the novelty consisted merely in the method of procuring *nitrous acid* (*pernitrous acid* of Gay-Lussac) in a separate state, and that the opinions considered by Gay-Lussac as new had been previously entertained by chemists in general. In proof of this I quoted what I had myself stated on the subject in papers formerly written. M. Gay-Lussac has thought proper to quote what I said (Ann. de Chim. et Phys. ii. 182), and to draw as an inference that I claimed his discoveries, as having been previously made by myself. Nothing was further from my thoughts than laying any such claim. My object was merely to show that the opinions of Gay-Lussac had been already adopted by chemists before his paper appeared. I might have quoted other books on the subject if it had been requisite, some of them of rather an old date. Let him consult Davy's Researches, p. 30; or Dobereiner's paper in Schweigger's Journal, xiv. 219; and he will see that my opinion respecting *nitrous vapour* is neither new nor singular.

M. Dulong has lately shown, by direct experiment, that what Gay-Lussac considered as pernitrous acid is the same as the nitrous acid vapour of former chemists, and that his pernitrous acid may be formed directly by uniting together oxygen and nitrous gas. (See Ann. de Chim. et Phys. ii. 317.)

3. *Specific Gravity of Gases.*—In the *Annals*, vii. 343, I have given a table of the specific gravity of gaseous bodies. Since that time M. Gay-Lussac has published a still fuller table (Ann. de Chim. et Phys. i. 218). His numbers differ very little from those which I had previously given; and the chief cause of the difference is perhaps owing to my having reduced all the densities by calculation to the temperature of 60°, which Gay-Lussac seems to have neglected. I shall insert his table here for the gratification of my readers.

Gaseous Bodies.	Sp. Gr. by exper.	Ditto calculated.	Experimenters.
Air	1·0000		
Vapour of iodine		8·6195	Gay-Lussac. Ann. de Chim. xci. 17.
Vapour of hydriodic ether	5·4749		Gay-Lussac.
Vapour of oil of turpentine	5·0130		Gay-Lussac.
Hydriodic acid gas	4·4430	4·4288	Gay-Lussac. Ann. de Chim. xci. 16.
Fluosilicic acid gas	3·5735		John Davy. Phil. Trans. 1812, p. 354.
Phosgene gas		3·3894	Id. Ib. p. 150.
Nitrous acid gas		3·1764	Gay-Lussac.
Vapour of sulphuret of carbon	2·6447		Gay-Lussac.
Vapour of sulphuric ether	2·5860		Gay-Lussac.
Chlorine	2·4700	2·4216	Gay-Lussac and Thenard.
Euchlorine		2·3144	Gay-Lussac.
Fluoboric gas	2·3709		John Davy. Phil. Trans. 1812, p. 366.
Vapour of muriatic ether	2·219		Thenard. Mem. d'Arceuil, i. 121.
Sulphurous acid gas	2·1930	2·2072	Davy.
Chloro-cyanic vapour		2·1113	Gay-Lussac. Ann. de Chim. xcv. 210.
Cyanogen	1·8064	1·8011	Id. Ib. p. 177.
Vapour of absolute alcohol	1·6133	1·6030	Gay-Lussac.
Nitrous oxide	1·5204	1·5209	Colin.
Carbonic acid	1·5196		Biot and Arago. Mem. de l'Instit. 1806, p. 320.
Muriatic acid	1·2474	1·2505	Id. Ib. p. 320.
Sulphureted hydrogen	1·1912	1·1768	Thenard and Gay-Lussac. Recherch. Phys.-Chim. i. 191.
Oxygen	1·1036		Biot and Arago. Mem. de l'Instit. 1806, p. 320.
Nitrous gas	1·0388	1·0364	Berard.
Olefiant gas	0·9784		Th. de Saussure. Ann. de Chim. lxxxix. 283.
Azote	0·9691		Arago and Biot. Mem. de l'Instit. 1806, p. 320.
Oxide of carbon	0·9569	0·9678	Cruickshanks.
Hydro-cyanic vapour	0·9476	0·9360	Gay-Lussac. Ann. de Chim. xcv. 150.
Phosphureted hydrogen	0·870		Davy.
Steam	0·6235	0·6250	Gay-Lussac.
Ammonia	0·5967	0·5943	Biot and Arago. Mem. de l'Instit. 1806, p. 320.
Carbureted hydrogen	0·5550	0·5624	Thomson.
Arseniated hydrogen	0·5290		Trommsdorf.
Hydrogen	0·0732		Arago and Biot. Mem. de l'Instit. 1806, p. 320.

4. *Relation between the Specific Gravity of Gaseous Bodies and the Weights of their Atoms.*—In the *Annals*, vii. 343, I published a short paper on this subject, in which I endeavoured to follow out an idea that had been first started by Dr. Prout. I showed that if we make the specific gravity of oxygen gas 1, and refer that of all

the other gases to it, in that case gaseous bodies may be reduced under three classes. In the first class the specific gravity of the gas and the weight of its atom are represented by the same number. In the second class the weight of the atom is double the specific gravity; and in the third class the weight of the atom is four times the specific gravity of the respective gases.

5. *Atom of Strontian.*—By a set of experiments given in the *Annals*, vii. 399, I determined the weight of an atom of strontian to be 6·449. Probably the true number is 6·500, which would make an atom of strontian 52 times heavier than an atom of hydrogen.

VI. LIGHT.

1. *Phosphorescence of Bodies.*—Beccaria's curious experiments on the light emitted by most bodies when suddenly carried into a dark place after being exposed to the direct rays of the sun, which were published many years ago in the Memoirs of the Bologna Academy, are known, I presume, to most of my readers. Since that time several additional facts have been brought to light, particularly by Mr. Canton, who contrived a substance possessed of this quality in an eminent degree. Theodore von Grotthus has lately made known a natural substance which possesses this phosphorescent property in a much higher degree than any other hitherto observed. He has published a detailed description of the phenomena exhibited by this new body, and has at the same time contrived an elaborate theory, which he considers as affording an explanation of phosphorescence in general. (Schweigger's Journal, xiv. 133.) The substance in question is the *reddish violet fluor spar from Nertschinsk*, belonging to that variety of fluor spar long known to mineralogists under the name of *chlorophane*. This substance, when slightly heated, gives out a copious emerald-green colour. Even the heat of the hand is sufficient to produce the effect. If it be exposed to the light of the sun, or of a candle, and afterwards taken into a dark place, it emits light, and continues to do so for a long time. Grotthus compared it thus circumstanced with Canton's pyrophorus, and found it to shine much longer, and with more brilliancy. His theory of phosphorescence is, that the solar light upon the surface of the phosphorescent body between its elementary poles is decomposed into its elementary electrical principles, namely, plus and minus electricity, and that the subsequent union and escape of these elements of light occasion the phosphorescence of the body. This hypothesis the author illustrates at great length, and endeavours to show that it agrees with the experiments and observations of Dessaignes when properly interpreted. I have not room here to examine in detail the grounds upon which this hypothesis is supported; nor indeed can I say that I fully understand it. I conceive it will be sufficient to refer those readers who wish to go into the subject to the paper itself. It is entitled, Ueber einen neuen Lightsauger nebst einigen allgemeinen Betrachtungen

über die Phosphorescenz und die Farben. (Schweigger's Journal, xiv. 133; xv. 172.)

2. *Homberg's Pyrophorus.*—About seven years ago Sir H. Davy announced that Homberg's pyrophorus owes its peculiar properties to a quantity of potassium reduced during the formation of the substance, and which evolves potassureted hydrogen when it comes in contact with moisture. Some years ago Dr. Coxe, of Philadelphia, maintained a similar opinion in a letter which I published in an early number of the *Annals.* Dobereiner has lately noticed this opinion of Dr. Coxe, and endeavoured to show by some experiments that the substance formed, and to which the pyrophorus owes its peculiar properties, is a compound of potassium, sulphur, and carbon. (Schweigger's Journal, xvi. 118.)

VII. HEAT.

1. *Dilatation of Bodies by Heat.*—MM. Gay-Lussac and Arago have published in the Ann. de Chim. et Phys. various tables of the dilatation of bodies by heat. I shall insert here such of these tables as are not already familiarly known to the English reader.

TABLE I.—*Linear Dilatation of different Substances when raised from the Temperature of freezing Water to the boiling Heat of that Liquid, according to the Experiments of Laplace and Lavoisier.*

Substances.	Dilatation for a length = 1.	
	In Decimals.	In Vulgar Fractions.
Steel not tempered	0·00107915	$\frac{1}{927}$
Steel tempered, and then heated to 150°	0·00123956	$\frac{1}{807}$
Silver from the cupel	0·00190974	$\frac{1}{524}$
Silver, Paris standard	0·00190868	$\frac{1}{524}$
Copper	0·00171733	$\frac{1}{582}$
Brass	0·00187821	$\frac{1}{532}$
Tin from Malacca	0·00193765	$\frac{1}{516}$
Tin from Cornwall	0·00217298	$\frac{1}{462}$
Hammered iron	0·00122045	$\frac{1}{819}$
Iron wire	0·00123504	$\frac{1}{812}$
English flint glass	0·00081166	$\frac{1}{1248}$
Mercury (in volume)	0·01847746	$\frac{1}{54 \cdot 12}$
Fine gold	0·00146606	$\frac{1}{682}$
Gold, Paris standard, not annealed	0·00155155	$\frac{1}{645}$
Gold, Paris standard, annealed	0·00151361	$\frac{1}{661}$
Platinum (according to Borda)	0·00085655	$\frac{1}{1167}$
Lead	0·00284836	$\frac{1}{351}$
French crystal glass	0·00087199	$\frac{1}{1147}$
French crown glass	0·00089694	$\frac{1}{1115}$
French mirror glass	0·00089089	$\frac{1}{1122}$

During these experiments the authors observed that the dilatation of glass and of the metals was sensibly proportional to that of mercury; so that twice the number of degrees gives a double dilatation. Tempered steel alone presented striking exceptions to this law.

TABLE II.—*Linear Dilatations from 32° to 212°, according to the Experiments of Troughton.*

Steel	0·0011899	= $\frac{1}{840}$
Silver	0·0020826	= $\frac{1}{480}$
Copper	0·0019188	= $\frac{1}{521}$
Iron wire	0·0014401	= $\frac{1}{694}$
Platinum	0·0009918	= $\frac{1}{1008}$
Palladium (according to Wollaston)	0·0010	= $\frac{1}{1000}$

TABLE III.—*Dilatation of Liquids in Volume between 32° and 212°, according to Dalton.*

Muriatic acid	0·0600	= $\frac{1}{17}$
Nitric acid	0·1100	= $\frac{1}{9}$
Sulphuric acid	0·0600	= $\frac{1}{17}$
Alcohol	0·1100	= $\frac{1}{9}$
Water	0·0466	= $\frac{1}{22}$
Water saturated with common salt	0·0500	= $\frac{1}{20}$
Ether	0·0700	= $\frac{1}{14}$
Fixed oils	0·0800	= $\frac{1}{12}$
Oil of turpentine	0·0700	= $\frac{1}{14}$
Mercury	0·0200	= $\frac{1}{50}$
Mercury according to Lord C. Cavendish	0·01872	= $\frac{1}{53}$

TABLE IV.—*Dilatation of Liquids.*

Gay-Lussac has lately made a set of experiments on the dilatation of liquids, in order, if possible, to discover the law by which this dilatation is regulated. The experiments were made in thermometer tubes hermetically sealed. The boiling point of each liquid was pitched upon as the zero, and the degrees were reckoned from that point downwards according to the centigrade scale. The liquids pitched upon were water, alcohol, sulphuret of carbon, and sulphuric ether. Their boiling points were as follows:—

Water	100°	or	212° Fahr.
Alcohol	78·41	....	173
Sulphuret of carbon	46·60	....	126
Sulphuric ether	35·66	....	96

The following table exhibits the contractions which took place in the volume of these bodies respectively when exposed to different temperatures below their boiling points:—

Water.		Alcohol.		Sulphuret of Carbon.		Sulphuric Ether.	
Temp.	Contract.	Temp.	Contract.	Temp.	Contract.	Temp.	Contract.
0·0	0·00	0·0	0·00	0·0	0·00	0·0	0·00
3·6	2·44	4·4	4·90	1·3	1·59	1·3	2·08
8·0	5·40	5·5	6·08	3·6	4·38	2·6	4·04
9·2	6·13	6·7	7·59	5·0	6·14	4·4	7·18
14·3	10·13	11·6	13·25	7·9	9·67	6·1	9·88
21·0	13·68	15·2	17·82	10·1	12·12	7·7	12·46
26·6	17·00	19·8	23·13	12·4	14·93	9·1	14·74
33·1	20·53	23·6	27·52	15·0	17·98	10·7	17·33
39·9	24·06	26·8	31·15	17·8	21·20	12·2	19·76
46·2	26·95	31·8	36·79	20·4	24·27	14·0	22·65
51·4	29·14	34·8	40·05	22·9	27·10	16·9	27·06
56·4	31·16	40·8	46·57	25·0	29·65	20·3	32·27
61·5	32·94	47·9	53·81	27·3	31·98	21·1	33·46
67·4	34·76	51·9	57·92	29·8	34·84	25·9	40·37
72·2	36·07	56·7	62·74	31·1	36·27	28·8	44·69
76·1	36·94	61·2	67·15	33·3	38·68	30·3	45·47
78·7	37·45	62·9	68·88	35·7	41·20	31·0	47·81
80·2	37·74	63·5	69·33	37·4	43·01	31·1	47·88
60·4	37·80	65·5	71·16	38·1	43·68	34·0	50·72
84·5	38·25	67·3	72·97	41·0	46·85	37·3	55·25
86·0	38·52	70·7	76·10	42·3	48·11	39·9	58·54
		72·5	77·85	44·7	50·68	40·5	59·56
		73·8	79·03	47·7	53·94	48·2	69·67
				50·0	56·28	51·6	74·04
				51·1	57·39	53·1	75·87
				61·7	67·83	54·3	77·45
				63·3	69·43	54·7	77·90
				64·3	70·45	55·4	78·84

In order the better to show the rate of dilatation, the following table was calculated, showing the degree of contraction for every five centigrade degrees below the boiling point of each liquid.

	Water		Alcohol.		Sulphuret of Carbon.		Ether.	
	Contract by exper.	Ditto calculated.	Contract by exper.	Ditto calculated.	Contract by exper.	Ditto calculated.	Contract by exper.	Ditto calculated.
0°	0·00	0·00	0·00	0·00	0·00	0·00	0·00	0·00
5	3·34	3·35	5·55	5·56	6·14	6·07	8·15	8·16
10	6·61	6·65	11·43	11·24	12·01	12·08	16·17	16·01
15	10·50	9·89	17·51	17·00	17·98	17·99	24·16	23·60
20	13·15	13·03	24·34	23·41	23·80	23·80	31·83	30·92
25	16·06	16·06	29·15	28·60	29·65	29·50	39·14	38·08
30	18·85	18·95	34·74	34·37	35·06	35·05	46·42	45·04
35	21·52	21·67	40·28	40·05	40·48	40·43	52·06	51·86
40	24·10	24·20	45·68	45·66	45·77	45·67	58·77	58·57
45	26·50	26·52	50·85	51·11	51·08	50·70	65·48	65·20
50	28·56	28·61	56·02	56·37	56·28	55·52	72·01	71·79
55	30·60	30·43	61·01	61·43	61·14	60·12	78·38	78·36
60	32·42	31·96	65·96	66·23	66·21	64·48		
65	34·02	33·19	70·74	70·75				
70	35·47	34·09	75·48	74·93				
75	36·70	34·63	80·11	78·75				

From this table it appears that alcohol and sulphuret of carbon experience the same degree of dilatation. This, as Gay-Lussac has shown, depends upon the equal density of their vapours. He shows that

Alcohol at .... 78·41° produces 488·3 its volume of vapour at 100°
Sulph. of carb. 46·60 .... 491·1
Ether ...... 35·66 .... 285·9
Water ......100·00 ....1633·1

Gay-Lussac promises speedily to renew this interesting subject. (Ann. de Chim. et Phys. ii. 130.)

2. *Heat evolved by Combination.*—It was an opinion entertained by Dr. Irvine that whenever two bodies unite together, a quantity of heat is evolved. This opinion was founded chiefly upon mixtures of sulphuric acid and water, and alcohol and water. Upon these mixtures Dr. Irvine appears to have founded a great part of his peculiar doctrines respecting heat. It was observed that in all these cases the density of the mixture was greater than the mean. Hence it was concluded that whenever two bodies unite so that the density increases, heat is evolved; but when the density diminishes, heat is absorbed. Gay-Lussac has lately published several facts which he considers as inconsistent with this doctrine. (Ann. de Chim. et Phys. ii. 214.) Most of these facts were previously known. They are as follows:—

(1.) A saturated solution of nitrate of ammonia, at the temperature of 61°, and of the density 1·302, was mixed with water in the proportion of 44·05 to 33·76. The temperature of the mixture sank 8·9°; but the density at 61° was 1·159, while the mean density was only 1·151.

(2.) On adding water to the preceding mixture in the proportion of 33·64 to 39·28, the temperature sank 3·4°, while the density continued 0·003 above the mean.

Other saline solutions present the same result, though none to so great a degree.

(3.) The chloride of azote gives out heat and light when decomposed, and reduced to the two simple bodies—chlorine and azote.

(4.) Iodide of azote likewise gives out heat and light when reduced into iodine and azote.

(5.) Euchlorine detonates at a temperature below 212°, and gives out heat and light when reduced into chlorine and oxygen.

3. *Division of Fahrenheit's Scale.*—My readers will have perused with interest some curious particulars respecting Fahrenheit's thermometrical scale printed in the *Annals*, viii. 26, for which I am obliged to an anonymous correspondent. The paper in question having made its appearance so recently, I conceive it to be unnecessary to give any details from it here.

4. *Dr. Marcet's Method of producing a violent Heat.*—In the *Annals*, ii. 99, Dr. Marcet published a method of producing a very violent heat. It consisted in passing a current of oxygen gas through

the flame of a spirit lamp. This method has been lately tried by Professor Stromeyer, of Gottingen, and he has obtained by means of it some unexpected results. (Schweigger's Journal, xv. 270.) Platinum wire 1·75 millimetres in diameter melted very speedily. When its diameter was only 0·5 millimetre, it burned brilliantly. Iron wire several millimetres in diameter melted rapidly, and burned; and a watch-spring burned with the same brilliancy as it would have done in oxygen gas. Rock crystal and common quartz in small fragments melted completely into a glass bead. The same experiments were tried with lime and magnesia; but the success was not so complete. The surface was converted into an enamel, and the sharp edges were blunted; so that Stromeyer has little doubt that he will be ultimately able to fuse both of these bodies, hitherto considered to be completely refractory.

VIII. SIMPLE SUPPORTERS.

1. *Oxygen and chlorine.*—Hitherto only three compounds of oxygen and chlorine have been discovered; namely, 1. *Proto-chlorous oxide*, or the *euchlorine* of Davy, composed of one atom chlorine and one atom oxygen. 2. *Deuto-chlorous oxide*, the new gas discovered by Davy, and described in the Phil. Trans. for 1815. (See *Annals*, vii. 28.) According to his experiments it is composed of one atom chlorine and four atoms oxygen. 3. The *chloric acid* of Gay-Lussac, obtained by decomposing chlorate of barytes by means of sulphuric acid, and composed, according to his experiments, of one atom chlorine and five atoms oxygen. In Gilbert's Annalen, lii. 197, or the second number of that work for 1816, there is a curious paper, by Frederick, Count of Stadion, in Vienna, On the Combinations of Chlorine and Oxygen. He does not appear to have been acquainted with the late experiments of Davy; but he discovered the *deuto-chlorous oxide* nearly in the same way that Davy had done. His method was to fuse a small quantity of chlorate of potash in a retort, to allow it to cool, and then to pour over it concentrated sulphuric acid. This mixture being exposed for three hours to the heat of a water-bath gradually raised from 54·5° to 212°, the new gas came over, and was received over mercury. Its properties were as follows:—

It has a lively-yellow colour, much more intense than that of proto-chlorous oxide. Its smell is quite peculiar, and does not occasion catarrh, as is the case with chlorine. It does not alter blue paper. It may be preserved unaltered in the dark, provided it be not in contact with combustible or alkaline bodies; but when exposed to the rays of the sun, its bulk is increased, and it is decomposed into chlorine and oxygen. Heat and electric sparks occasion the same decomposition. When raised to a temperature between 112° and 144°, it explodes. It explodes, likewise, when an electric spark is passed through it. When thus decomposed over mercury, the chlorine unites with the mercury, and leaves a quantity of oxygen equal to the original bulk of the gas. From other experiments it follows that, after decomposition, the bulk of the chlorine was to that of the oxygen as two to three. From this it follows that deuto-chlorous oxide is composed of one atom chlorine and three atoms oxygen. Hence it is probably a different substance from the gas examined by Davy. When deuto-chlorous gas and hydrogen are mixed, no change takes place at the common temperature of the atmosphere; but when an electric spark is passed through the mixture, a detonation takes place, and the whole is converted into muriatic acid and water. According to Count von Stadion's experiments, three measures of deuto-chlorous gas require eight measures of hydrogen. This is the proportion that ought to follow from the composition of deuto-chlorous gas as determined by Stadion's experiments.

Water absorbs at least seven times its volume of deuto-chlorous oxide. The solution has a deep yellow colour, a peculiar pungent taste, and the distinguishing odour of deuto-chlorous oxide. It may be kept in the dark in close vessels without undergoing any change, but when exposed to the solar rays the oxide is decomposed, and converted into chlorine, and the chloric acid of Gay-Lussac. The colour disappears, and the liquid assumes the smell of chlorine. When heat is applied, the chlorine is driven off, and pure chloric acid remains behind. The evaporation must be conducted at a temperature between 112° and 144°, and be continued till about one-fourth of the liquid is driven off. The residue has then lost the odour of chlorine, and does not precipitate nitrate of silver. The deuto-chlorous gas is decomposed in the same way, but more slowly, when left in contact with alkaline bases, or with metals.

When deuto-chlorous oxide is procured by this process, there is a peculiar salt formed, which has been hitherto overlooked. This salt is best obtained by adding three or four grains of strong sulphuric acid for every grain of chlorate of potash employed. After the first violent action of the acid is at an end, heat is to be applied, and continued till the yellow colour of the mass disappears. The salt formed in this way is mixed with bisulphate of potash, which may be separated by a second crystallization. This salt possesses the following properties:—

It is quite neutral, is not altered by exposure to the air, and has a weak taste, similar to that of muriate of potash. It dissolves in considerable quantity in boiling water; but water at the temperature of 60° dissolves only $\frac{1}{55}$th part of its weight. In alcohol it is quite insoluble. Its crystals appear to be octahedrons, and to belong to the variety distinguished by Haüy under the name *plomb sulfaté semi-prismé*, and figured by him in his 69th plate, fig. 73. It detonates feebly when triturated in a mortar with sulphur. When heated to 412°, it is decomposed, and converted into chloride of potassium (*muriate of potash*) and oxygen gas. When this salt is mixed with its own weight of sulphuric acid, and exposed to a heat of 280° in a retort, it is decomposed, and the acid which it contains may be distilled over. The same acid may be formed artificially by exposing deuto-chlorous oxide to the action of voltaic electricity in an apparatus constructed with platinum wires. According to the analysis of Count von Stadion, when this salt is exposed to heat it is converted into

Potassium	2·849	5·408
Chlorine	2·559	
Oxygen		4·592

Now 2·849 grains of potassium require, in order to be converted into potash, 0·5819 grain of oxygen. There remain 4·01 grains. Hence the acid must be composed of

Chlorine	2·559
Oxygen	4·01

But these two numbers are to each other very nearly as 4·5 (an atom of chlorine) to 7 (7 atoms of oxygen). Hence it follows that this acid is composed of one atom of chlorine and seven atoms of oxygen. Count von Stadion calls it *oxy-chloric acid.* It may perhaps be better to give it the name of *perchloric acid.* Thus it appears that no fewer than four combinations of chlorine and oxygen exist, namely,

	Chlorine.		Oxygen.
Proto-chlorous oxide, composed of	1 atom	+	1 atom
Deuto-chlorous oxide	1	+	3 ?
Chloric acid	1	+	5
Perchloric acid	1	+	7

So that an uneven number of atoms of oxygen, it would appear, always unite with an atom of chlorine.

IX. SIMPLE COMBUSTIBLES.

1. *Boron.*—The present method of preparing boron is both expensive and troublesome. Döbereiner has proposed another, which is at least cheaper. (Schweigger's Journal, xvi. 116.) Borax is fused, reduced to a fine powder, and mixed with the tenth part of its weight of lamp-black. The mixture is put into a gun-barrel, and exposed for two hours to a white heat. Abundance of carbonic oxide is driven off, indicating a decomposition of the boracic acid. After the process, a compact mass remains, of a greyish-black colour. This mass, being pounded, and repeatedly washed with hot water, and finally with muriatic acid, leaves a pulverulent substance, of a greenish-black colour, which possesses the properties of boron, excepting that it is mixed with a little charcoal.

Leopold Gmelin has made some attempts to combine boron with iron. A mixture of 10 parts iron filings and one part boracic acid was exposed in a Hessian crucible to the violent heat of an iron furnace. A metallic mass was obtained, which had obviously been fused. It possessed some ductility, was of a silver-white colour, and retained its magnetic virtues in perfection. This boruret of iron dissolved with difficulty in muriatic acid, and boreted hydrogen gas was disengaged. But the gas obtained by Gmelin seems to have been but imperfectly saturated with boron. It had the smell of hydrogen gas from iron, mixed with the odour of asafœtida, and burned with a green flame. Neither its specific gravity, nor the proportion of oxygen necessary to burn it completely, were ascertained. From some experiments on the borates, which will be stated in a subsequent part of this historical sketch, Gmelin concludes that the weight of an atom of boron is 5·8, and that boracic acid is composed of 1 atom boron + 2 atoms oxygen, or of 74·4 boron and 25·6 oxygen. (Schweigger's Journal, xv. 245.)

2. *Charcoal.*—According to Dobereiner, wood charcoal, after being exposed to a red heat, is a compound of

Carbon	68·4
Hydrogen	1·0

Before exposure to a red heat, its composition is—

Carbon	68·4
Hydrogen	1·5

(Schweigger's Journal, xvi. 92.) According to the same chemist, animal charcoal is composed of six atoms carbon and one atom azote. (Ibid. 86.)

3. *Gas from Coal.*—Lampadius has published a set of experiments on the quantity of gas obtained by the distillation of various kinds of German coal. The proportions and illuminating qualities vary considerably; but I do not consider it as worth while to state the results, because no description whatever is given of the varieties of coal employed. The experiments of course can be useful only to those who are acquainted with the nature of the coals which Lampadius used. For their advantage, I may mention that the experiments in question are inserted in Schweigger's Journal, xv. 142, published on Jan. 20, 1816.

Mr. Brande has given some useful and amusing facts respecting the gas from pit coal considered as a substitute for oil. (Journal of the Royal Institution, i. 71.) A chaldron of good Wallsend Newcastle coals yields from 17,000 to 20,000 cubic feet of gas; but in large establishments the quantity obtained seldom exceeds 12,000 cubic feet. At the three stations belonging to the chartered Gas Light Company, situated in Peter-street, Westminster, Worship-street, and Norton Falgate, 25 chaldrons of coals are carbonized daily, which yield 300,000 cubic feet of gas, equal to the supply of 75,000 Argand's lamps, each giving the light of six candles. At the City Gas Works, in Dorset-street, Blackfriars Bridge, the daily consumption of coals amounts to three chaldrons, which afford gas for the supply of 1500 lamps: so that the total consumption of coals daily in London for the purpose of illumination amounts to 28 chaldrons, and the number of lights supplied to 76,500.

4. *Olefiant Gas.*—In the year 1811 I published in the first volume of the Memoirs of the Wernerian Society of Edinburgh a paper on the gaseous combinations of carbon and hydrogen. In that paper (p. 516) I detailed some experiments which I made on the oily-looking substance formed by the action of chlorine on olefiant gas. From these experiments I drew as a conclusion that the substance in question was not an oil, but a compound of olefiant gas and chlorine. This conclusion has been recently confirmed by the experiments of Robiquet and Colin (Ann. de Chim. et Phys. i. 337), who prepared considerable quantities of the liquid in question, and examined its properties considerably in detail. They obtained it by passing a current of chlorine and olefiant gas (both pure) into a large glass globe. The liquid was freed from its excess of chlorine, if any happened to be present, by washing it with a little distilled water. Thus prepared, it possessed the following properties:—

It is colourless as water. It has an agreeable odour, very similar to that of muriatic ether. Its taste is sweet, sharp, and rather agreeable. Its specific gravity, at the temperature of 44°, is 1·2201, that of water being unity. The density of its vapour, at the temperature of 48·7°, is such that it supports a column of mercury 24·666 inches in length. Hence its boiling point is 152°. The specific gravity of its vapour, according to the experiment of Gay-Lussac, is 3·4434. It burns with a green flame, emitting a thick smoke, and depositing a good deal of charcoal. It is composed of one volume of chlorine and one volume of olefiant gas condensed into one volume. Now the specific gravity of

Chlorine is	2·500
Olefiant gas	0·974
	3·474

Thus we see that the oil is equal to the specific gravities of these two gases united, as it ought to be.

Muriatic ether, on the other hand, is composed of 1 volume of muriatic acid + 1 volume of olefiant gas condensed into one volume. Hence the reason of its greater volatility, and less specific gravity. The supposed oil of the Dutch chemists, then, is *chloric ether*; or, if a systematic name should be preferred, it may be called *chloride of olefiant gas.*

5. *Arsenical Hydrogen Gas.*—It is sufficiently known to chemists that it was during a set of experiments on the preparation of this gas that Gehlen lost his life. Professor Schweigger has lately made us acquainted with the process which he followed. It consisted in heating a mixture of arsenic in the state of powder and a concentrated alkaline ley. (Schweigger's Journal, xv. 501.)

6. *Phosphureted Hydrogen.*—In a late number of the *Annals* I published a set of experiments on this gas, hitherto but little examined. It may be obtained by putting pieces of phosphuret of

lime into a retort previously filled with water, or still better with water acidulated with muriatic acid.

This gas is colourless. It has a smell somewhat similar to that of onions. Its taste is intensely bitter. Its specific gravity at 60° is 0·9022. Water absorbs about $\frac{1}{50}$th of its bulk of this gas, and acquires an intensely bitter taste, and the property of precipitating several metalline solutions. It burns when it comes in contact of air in a wide vessel; but in a narrow tube a white smoke is produced, and the whole of the phosphorus disappears. One volume of phosphureted hydrogen, and half a volume of oxygen, mixed in this manner, leave one volume of pure hydrogen. When electric sparks are passed through it, the phosphorus is deposited, and pure hydrogen remains, equal in bulk to the original gas. Hence it is composed of hydrogen holding phosphorus in solution. The proportions are one part by weight of hydrogen and 12 parts by weight of phosphorus. Hence an atom of phosphorus weighs 1·5.

Phosphureted hydrogen gas requires for complete combustion either one volume or $1\frac{1}{2}$ volume of oxygen gas. In the first case phosphorous acid, in the second phosphoric acid, is formed. Hence phosphorous acid is composed of

Phosphorus	100 or	3
Oxygen	66·6	2

and phosphoric acid of

Phosphorus	100 or	3
Oxygen	133·3	4

For complete combustion, phosphureted hydrogen requires three volumes of nitrous gas. Phosphoric acid and water are formed, and $1\frac{1}{2}$ volume of azote remains. The same proportion of oxide of azote is necessary; the same substances are formed, and three volumes of azote remain. Three volumes of chlorine and one volume of phosphureted hydrogen, when mixed over water, completely disappear, muriatic acid and bichloride of phosphorus being formed. Iodine decomposes this gas likewise, and forms a white substance, which is iodide of phosphorus, while hydrogen gas remains. Four grains of iodine are requisite to decompose $1\frac{1}{2}$ cubic inch of phosphureted hydrogen gas. This gas is composed of 1 atom hydrogen + 1 atom phosphorus. There is another gas composed of 2 atoms hydrogen + 1 atom phosphorus. The first should be called *hydroguret of phosphorus*; the second *bihydroguret of phosphorus.*

We might indeed consider the gas which I have analyzed as composed of 1 atom hydrogen + 2 atoms phosphorus, and the other gas as a compound of 1 atom hydrogen + 1 atom phosphorus. But I do not see how this supposition could be made to agree with the numerous experiments which have been made on the composition of phosphoric acid. On such a supposition, an atom of phosphorus would weigh 0·75, which is the same weight as that of an atom of carbon.

7. *Carburet of Phosphorus.*—This is a tasteless substance, of a lemon-yellow colour. It is probably gradually acidified by exposure to the air; at least I find that it attracts moisture. It does not melt when heated, but burns with considerable splendour. A red heat drives off the carbon, and leaves the phosphorus. It is composed of 1 atom phosphorus + 1 atom carbon. It is obtained when phosphuret of lime is dissolved in muriatic acid.

8. *Phosphuret of Potash.*—Professor Sementini, at Rome, has announced the discovery of a compound of phosphorus and potash. His experiments were published in the *Annals*, vii. 280. It was obtained by leaving pieces of phosphorus in a saturated solution of potash in alcohol. Brilliant scales were gradually deposited at the bottom of the vessel. These scales constituted the substance in question.

9. *Composition of Alcohol and Ether.*—Gay-Lussac has rendered it very probable (Ann. de Chim. xcv. 311) that alcohol is composed of

One volume olefiant gas,
One volume vapour of water,

condensed into one volume. This is the same thing as to say that alcohol is a compound of 2 atoms of olefiant gas + 1 atom of water.

Ether, according to him, is composed of

Two volumes olefiant gas,
One volume of vapour of water,

condensed into one volume. It is, therefore, a compound of 4 atoms olefiant gas + 1 atom water.

10. *Sulphuric Ether.*—Gay-Lussac has observed that when sulphuric ether is kept for some time in vessels containing a good deal of air, and occasionally opened, it undergoes a spontaneous alteration. Acetic acid makes its appearance in it, and a peculiar oil, which seems to have the property of combining and forming a solid detonating compound with muriatic acid. (Ann. de Chim. et Phys. ii. 98.) M. Planche had previously observed the evolution of acetic acid in ether kept under the circumstances pointed out by Gay-Lussac. (Ibid. 213.)

11. *Strength of Wine.*—Mr. Brande lately examined a Greek wine called Lissa, imported into this country, and likewise some genuine Marsala from Sicily. The quantity of alcohol contained in 100 parts of each he found as follows:—

Lissa	26 to 24
Marsala	26·3 to 25·5

These, therefore, are the strongest wines hitherto examined.—(Journal of the Royal Institution, i. 136.)

X. METALS.

1. *Gold.*—The solution of gold in aqua regia has long been familiarly known to chemists, though the examination of it has been hitherto so difficult that chemists contradict one another respecting the effect produced by the addition of alkalies. According to Vauquelin, Duportal, and Pelletier, the alkalies do not occasion a precipitation when poured into this liquid cold. Oberkampf obtained a contrary result. According to M. Figuier, a precipitation always takes place, and it makes no difference whether the liquid contains an excess of acid or not; except that in the former case a greater proportion of alkali is requisite; for the precipitate never appears unless there be an excess of alkali. Six grammes of dry muriate of gold were dissolved in 150 grammes of distilled water. The solution was divided into two equal parts, and put into two conical glasses. Into the one were put four grammes of muriatic acid. Both were saturated with a solution of caustic potash. The colour became red, and a grey-coloured flocky precipitate gradually fell. This precipitate was at its maximum in 48 hours. It was the same in each glass. Being separated by the filter, the liquids were heated. New precipitates fell, much darker in their colour. The whole being united and dried, were found to represent in each two thirds of the gold held in solution. On adding muriatic acid to each solution, the yellow colour was restored. Potash occasioned a new precipitate in each; and by adding muriatic acid and potash alternately, the whole gold was precipitated. (Ann. de Chim. et Phys. ii. 102.) How is this precipitation to be explained?

2. *Purification of crude Platinum.*—The Marquis of Ridolfi has tried the following method of separating the foreign metals with which crude platinum is alloyed. He fuses it with half its weight of lead, reduces the alloy to powder, mixes it with sulphur, and exposes it to a strong heat in a covered crucible. A brittle button was obtained, consisting of platinum, lead, and sulphur. It was fused with a small addition of lead, heated to whiteness, and hammered in that state with a hot hammer upon a hot anvil. By this means the lead was forced out in a state of fusion. The platinum thus obtained was ductile and malleable, and of the specific gravity 22·630. (Journal of the Royal Institution, i. 259.) From various trials which I have made, I have reason to believe that the specific gravity of pure platinum well hammered is 21·65. It is just possible, though not probable, that the specific gravity of the specimen examined by Ridolfi was increased by its being alloyed with a little lead.

3. *Cupellation of Silver.*—M. d'Arcet has published the following table, indicating from his own experiments the proportion of lead necessary for the cupellation of silver of different degrees of fineness. (Ann. de Chim. et Phys. i. 75.)

Composition of alloy.		Lead necessary to cupellate 1 of the alloy.	Ratio between the lead and the copper.
Silver.	Copper.		
1000	0	$\frac{3}{10}$	0 to 1
950	50	3	60 to 1
900	100	7	70 to 1
800	200	10	50 to 1
700	300	12	40 to 1
600	400	14	35 to 1
500	500	16 to 17	32 to 1
400	600	16 to 17	26·66 to 1
300	700	16 to 17	22·857 to 1
200	800	16 to 17	20 to 1
100	900	16 to 17	17·777 to 1
1	999	16 to 17	16·016 to 1
0	1000	16 to 17	16 to 1

The proportions used at our mint are, I have been told, much more economical than these.

4. *Mercury.*—A curious paper on the combination of mercury with oxygen and sulphur has been recently published by M. Guibourt. It is well known to chemists that two oxides of mercury exist; but M. Guibourt has shown that the protoxide cannot be obtained in a separate state. When the proto-chloride of mercury (*calomel*) is treated with an alkali, a black powder is obtained, which has been considered as protoxide of mercury. But when examined by a magnifying glass, globules of mercury may be detected in it. They become visible to the naked eye when the precipitate is pressed between two bodies. According to M. Guibourt, the protoxide of mercury is composed of

Mercury	100
Oxygen	4·5

and the peroxide of

Mercury	100
Oxygen	8

When sulphureted hydrogen gas is made to act upon proto-chloride of mercury, a black powder is obtained, which has been hitherto considered as a compound of mercury and sulphur. But M. Guibourt finds that globules of mercury become visible in it when it is compressed. On that account he does not consider it as a sulphuret. But as he finds it to be composed of 100 mercury + 8 sulphur, which corresponds exactly to an atom of each of these bodies, I do not see how we can refuse to consider it as a chemical compound. Cinnabar is formed when an excess of sulphureted hydrogen is mixed with deuto-chloride of mercury. It is composed of 100 mercury + 16 sulphur, or of 1 atom mercury + 2 atoms sulphur. (Ann. de Chim. et Phys. i. 422.)

5. *Steel.*—From the statements of Dobereiner and Goethe it would appear that iron is much better fitted for being converted into steel, when it contains manganese. (See Schweigger's Journal, xvi. 102.) The substance noticed by Dobereiner in the place just quoted, I consider as the crystallized body occasionally found in hollows of cast-iron, and known in this country by the name of geess. Dr. Wollaston informs me that he has examined this substance, and that he found it a carburet of manganese.

6. *Iron.*—In the *Annals*, vii. 320, I have given the result of an experiment lately made to determine the strength of British iron. It appears that an iron wire one inch in diameter is broken by a weight of 25·6 tons. Supposing Count Sickingen's experiments correct, the strength of Swedish and British iron is to each other as follows:—

British iron	348·88
Swedish iron	549·25

7. *Peroxide of Lead.*—Grindel has observed that when to a mixture of brown oxide of lead and sulphur a little phosphorus is added, and the mixture triturated in a mortar, a loud explosion takes place. (Schweigger's Journal, xv. 478.)

8. *Oxides of Iron.*—The general opinion at present entertained by chemists, is that iron combines only with two doses of oxygen, forming two oxides, the black and the red; that the black is composed of 100 iron + 30 oxygen, and the red of 100 iron + 45 oxygen. But this opinion is attended with some difficulties. When we compare the proportion of acid which combines with the black oxide with what unites with other salifiable bases, we find ourselves obliged to consider it as a protoxide. But in that case the red oxide presents the anomaly of one atom of iron combined with $1\frac{1}{2}$ atom of oxygen, unless we consider it as a compound of 2 atoms iron + 3 atoms oxygen. Gay-Lussac conceives that a third oxide of iron exists intermediate between the black and the red, and composed of 100 iron + 38 oxygen. It is formed by passing a current of steam over red-hot iron. (Ann. de Chim. et Phys. i. 33.) Were we to admit the existence of this oxide, it would be necessary to consider the weight of an atom of iron as 13·46. The black oxide would be composed of 1 atom iron + 4 atoms oxygen, the new oxide of 1 atom iron + 5 atoms oxygen, and the red oxide of 1 atom iron + 6 atoms oxygen. But this opinion is not very probable. I am more inclined to adopt the opinion of Berzelius, who considers the new oxide of Gay-Lussac as a compound of the black and the red oxide.

Gay-Lussac has shown by experiment that iron has the property of decomposing water at all temperatures up to a white heat, and that hydrogen in the same circumstances decomposes the oxide of iron. This constitutes an anomaly in the received doctrine of affinity similar to that pointed out by Mr. Phillips, and mentioned in a preceding part of this paper. I conceive that hydrogen has a greater affinity for oxygen than iron has, and that the decomposition of water by iron takes place only in a very limited degree.

9. *Oxides of Manganese.*—Berzelius in his first papers on the atomic theory admitted five oxides of this metal; but in his essay On the Cause of Chemical Proportions he reduces the number to four. (*Annals*, iii. 359.) Gay-Lussac admits only three; namely, the oxide obtained by dissolving manganese in acids, the peroxide which is found native, and the deutoxide formed by exposing the peroxide to a red heat. (Ann. de Chim. et Phys. i. 38.) I think it more probable that only two oxides of manganese exist; namely, the protoxide which dissolves in acids and forms salts, and the peroxide which exists native in such abundance. The brown oxide is more probably a compound of the protoxide and peroxide. It is incapable of dissolving in acids; and, when treated with acids, is resolved into protoxide and peroxide. The constituents of these oxides have not yet been determined in a satisfactory manner. Probably the protoxide is a compound of 100 metal + 20 oxygen, and the peroxide of 100 manganese + 40 oxygen. This would make the weight of an atom of manganese 5.

10. *Oxides of Tin.*—Berzelius has concluded from his experiments on tin that this metal combines with three doses of oxygen, and forms three oxides, all capable of uniting with acids. But Gay-Lussac has shown that we have no proofs that any difference exists between the deutoxide and peroxide. (Ann. de Chim. et Phys. i. 40.) The conclusion that only two oxides of tin exist removes an anomaly from the atomic doctrine, and is, therefore, preferable. Protoxide of tin is composed of

Tin	100	1 atom
Oxygen	13·6	1

and peroxide of

Tin	100	1 atom
Oxygen	27·2	2

This gives us the weight of an atom of tin 7·352. If we consider it as 7·375, it will then be a multiple of the weight of an atom of hydrogen, and the difference will be too small to be detected by experiment. The protoxide would be a compound of 100 tin + 13·56 oxygen, and the peroxide of 100 tin + 27·12 oxygen.

11. *Tantalum.*—Berzelius has lately subjected this metal to a much more complete examination than either Hatchett or Ekeberg had subjected it to. I have given a sketch of the results which he obtained in a late number of the *Annals*, viii. 233. Its colour is nearly similar to that of iron. Its specific gravity, when in a coherent mass, but not melted, is 5·61. It is brittle. It is not acted upon by any acid hitherto tried. When heated, it burns feebly, and is converted into a greyish-white oxide. It detonates feebly with nitre, and is converted into a snow-white oxide, which is com-

posed of 100 metal + 5·5 oxygen. If this be a protoxide, as is probable, the weight of an atom of tantalum is 18. This oxide possesses acid properties. It is precipitated from its combination with potash by muriatic acid. (See Afhandlingar, iv. 253.)

12. *Manufacture of Glass.*—Gehlen some time before his death was occupied with experiments on the preparation of glass by means of sulphate of soda. Professor Schweigger has lately published the result of his trials. (Schweigger's Journal, xv. 89.) He found that the following proportions were the best:—

Sand	100
Dry sulphate of soda	50
Dry quick-lime in powder	17 to 20
Charcoal	4

This mixture always gives a very good glass without any addition whatever. During the fusion the sulphuric acid is decomposed and driven off, and the soda unites with the silica. The sulphate of soda vitrifies very imperfectly when mixed alone with the silica. The vitrification succeeds better when quick-lime is added, and it succeeds completely when the proportion of charcoal indicated in the formula is added; because the sulphuric acid is thereby decomposed and dissipated. This decomposition may be either effected during the making of the glass, or before, at the pleasure of the workmen.

XI. ACIDS.

1. *Oxalic Acid.*—From the different analyses of oxalic acid which have been hitherto published, we may conclude that it contains twice as much oxygen in weight as of carbon, and that its hydrogen amounts to $\frac{1}{37}$th of the whole. Dobereiner has made a remark which is entitled to some attention. (Schweigger's Journal, xvi. 105.) The carbon and oxygen are precisely in the proportion which would result from the union of an atom of carbonic acid with an atom of carbonic oxide.

	Carbon.	Oxygen.	Carb.	Oxygen.
Carbonic acid is composed of	1 atom	+ 2 atoms or	0·75	+ 2
Carbonic oxide	1	+ 1	0·75	+ 1
			1·50	+ 3

When added together, we see that the weight of the oxygen is just double that of the carbon. Dulong says that when the oxalate of zinc or of lead is heated, it loses 20 per cent. of the weight of the acid; that afterwards, when the salt is exposed to a strong heat, no more water is driven off, but merely a mixture of carbonic acid and carbonic oxide, and that the metallic oxides remain behind in a state of purity. If we suppose oxalic acid to be composed of

Oxygen	3 atoms in weight	3
Carbon	2	1·5
Hydrogen	1	0·125

During the drying of these oxalates, the hydrogen of the acid must combine with oxygen, and form water. The weight of the water thus formed would be 1·125, or about 24 per cent. of the weight of the acid. The residual carbon and oxygen would be in the proportions proper for forming carbonic oxide. It would be impossible to resolve them into carbonic acid and carbonic oxide. Dobereiner conceives the hydrogen not to be an essential constituent of oxalic acid. But Dulong's experiments show us that hydrogen is invariably present. Oxalic acid has the property of reducing certain metallic oxides to the metallic state. In these cases water always makes its appearance during the decomposition of the acid. Were it not for the loss of weight stated to take place when oxalate of zinc and oxalate of lead are heated, it would be easy to explain the phenomena observed by Dulong. When the metallic oxide is not altered, the oxalic acid is resolved by heat into carbonic acid and carbonic oxide and hydrogen; but the hydrogen, in consequence of its small quantity, would escape detection. When the metallic oxide is decomposed, it supplies oxygen to the hydrogen of the acid, and converts it into water.

When a solution of oxalic acid in water is digested over black oxide of manganese, Dobereiner found that carbonic acid gas is given out. When sulphuric acid is poured upon the liquid, an additional quantity of carbonic acid is driven off. The carbonic acid thus formed is greater in weight than the whole of the oxalic acid employed. In this experiment the black oxide gives out sufficient oxygen to convert the hydrogen into water, and the whole of the carbon into carbonic acid.

2. *Aqua Regia.*—It has been generally supposed that aqua regia is a new acid formed by the combination of muriatic acid with nitric acid. But Sir H. Davy has lately examined the subject experimentally, and shown that this opinion is not well founded. When the two acids are mixed, they mutually decompose each other, water is formed, and chlorine and nitrous acid evolved. It is the chlorine thus disengaged that combines with gold and platinum, and occasions their solution. (Journal of the Royal Institution, i. 67.)

3. *Acids of Phosphorus.*—The acids formed by the combination of phosphorus with oxygen are of so difficult investigation, that hitherto we are but imperfectly acquainted with their composition. But the subject has lately attracted the attention of chemists in general; and so many persons have engaged in the investigation, that it cannot much longer remain incomplete. Two very curious and important papers have been published in the second volume of the Ann. de Chim. et Phys., the first by M. Dulong, the second by Professor Berzelius.

M. Dulong announces the existence of no fewer than four acids composed of oxygen and phosphorus, one of which he has had the honour of discovering himself. These acids he distinguishes by the following names: *hypophosphorous acid, phosphorous acid, phosphatic acid,* and *phosphoric acid.*

He obtained *hypophosphorous acid*, which is a discovery of his own, in the following manner. When phosphuret of barytes, of strontian, or of lime, is put into water, phosphureted hydrogen gas is disengaged, as is well known. The oxygen of the water decomposed unites to the phosphorus, and forms two acids, the hypophosphorous and the phosphoric, both of which unite to the base. The phosphate formed is insoluble in water, but the hypophosphite is very soluble. He treated phosphuret of barytes in this manner; and by filtrating the liquid, separated the phosphate of barytes. The liquid contained in solution hypophosphite of barytes. The barytes was precipitated by means of sulphuric acid, and nothing remained after filtration but the hypophosphorous acid united to water. This acid possesses the following properties. It has a sour taste, and does not crystallize. It may be concentrated by evaporation; and in that case we obtain a viscid liquid. When the heat is carried further, phosphureted hydrogen is driven off, a little phosphorus sublimes, and pure phosphoric acid remains behind. It absorbs oxygen slowly from the atmosphere. All the hypophosphites are very soluble in water. Those of barytes and strontian crystallize with difficulty. Those of potash, soda, and ammonia, are very soluble in alcohol. Hypophosphite of potash is much more deliquescent than muriate of lime. According to the analysis of Dulong, this acid is composed of

Phosphorus	100
Oxygen	37·44

But he suspects that it is a triple compound, and that it contains hydrogen.

Phosphorous acid was discovered by Davy. He obtained it by dissolving proto-chloride of phosphorus in water, and distilling off the muriatic acid. The phosphites have not hitherto been examined. They are less soluble than the hypophosphites; but the phosphite of potash is deliquescent and incrystallizable, and insoluble in alcohol. The phosphite of soda and of ammonia are likewise very soluble in water. The former crystallizes in rhomboids approaching nearly to the cube. Those of barytes, strontian, and lime, crystallize by spontaneous evaporation; but when their solutions are concentrated by heat, small pearly crystals precipitate, similar to acetate of mercury. These crystals are subphosphites, insoluble in water. Superphosphites difficultly crystallizable remain in solution in the liquid. According to the analysis of Dulong, this acid is composed of

Phosphorus	100
Oxygen	74·88

Phosphatic acid is obtained when phosphorus is allowed to burn slowly in the open air. M. Dulong considers it as a combination of phosphorous and phosphoric acids. According to him, it is always constant in its proportions. He considers it as composed of

Phosphorus	100
Oxygen	112·4

Phosphoric acid, which is obtained by the rapid combustion of phosphorus, has been long known. According to the analysis of Dulong, it is composed of

Phosphorus	100
Oxygen	124·8

This new acid discovered by M. Dulong is a chemical fact of considerable importance. If his hypophosphorous acid prove a combination of phosphorus and oxygen, without any hydrogen, it will oblige us to double the weight of an atom of phosphorus. I consider the method which I employed to determine the composition of phosphorus and phosphoric acids in my paper on phosphureted hydrogen gas as susceptible of much greater precision than any mode hitherto tried of analyzing the acids directly. On that account I conceive that the numbers which I have given are much nearer the truth than those of Dulong. The only part of my experiments liable to uncertainty is the specific gravity of the phosphureted hydrogen gas; for the quantities of gas which I weighed (25 cubic inches) were too small to give results of very great accuracy. Nevertheless I do not believe that I could fall into any great error, as I was at considerable pains to be as near precision as possible. If we correct Dulong's analyses by my experiments, we shall have the acids of phosphorus as follows:—

	Phosphorus.		Oxygen.
Hypophosphorous acid	100	+	$33{\cdot}\dot{3}$
Phosphorous acid	100	+	$66{\cdot}\dot{6}$
Phosphoric acid	100	+	$133{\cdot}\dot{3}$

I omit the phosphatic acid, because, even according to M. Dulong's own views, it cannot be considered as a simple combination of phosphorus and oxygen. If the hypophosphorous acid be a binary compound, the weight of an atom of phosphorus will be 3; but it would be premature to make any alteration in that weight till M. Dulong has published the experiments, which he promises, in order to investigate whether or not hydrogen be one of its constituents. Should this hypothesis be well founded, it must receive a new name, and be considered under quite a different point of view.

Professor Berzelius, one of the most accurate chemists of the present day, and certainly the most active, has lately published an elaborate set of experiments on the composition of phosphoric and phosphorous acids. To determine the composition of phosphoric acid, he digested a given weight of phosphorus in a solution of gold in aqua regia, evaporated to dryness, and re-dissolved in water. 0·754 of phosphorus reduced 7·93 of gold to the metallic state; or one part of phosphorus to be converted into phosphoric acid unites with as much oxygen as is capable of combining with 10·517 parts of gold. But 100 gold, according to Berzelius, combines with 12·08 oxygen. Therefore, according to this experiment, phosphoric acid is composed of

Phosphorus	100
Oxygen	126·94

In another experiment 0·8115 of phosphorus reduced 13·98 parts of silver from the sulphate to the metallic state. Hence one part of phosphorus combines, in order to be converted into phosphoric acid, with all the oxygen capable of uniting with 17·227 parts of silver. But 100 silver combine with 7·291 of oxygen. (See *Annals*. iv. 15.) Therefore, according to this supposition, phosphoric acid is composed of

Phosphorus	100
Oxygen	125·53

The mean of these two experiments gives us 126·23 for the quantity of oxygen combined with 100 phosphorus in phosphoric acid. These results agree very nearly both with the experiments of Dulong and my own, made by burning phosphorus in an apparatus contrived for the purpose. But I do not see how they can be reconciled with the composition of phosphoric acid as determined by the combustion of phosphureted hydrogen gas, which appears to be the simplest and most decisive method of determining the point.

Phosphorous acid, according to the calculation of Berzelius, is composed of

Phosphorus	100
Oxygen	76·92

But this calculation is founded on data that appear, to say the least of them, very questionable. Such a result cannot be reconciled with the undoubted fact that the oxygen requisite to convert the phosphorus in phosphureted hydrogen into phosphorous acid is just one half of what is required to convert it into phosphoric acid. According to Berzelius's estimates, the oxygen in phosphorous acid is to that in phosphoric acid as 3 : 5. According to this estimate, the weight of an atom of phosphorus would be 3·901. Further researches are necessary before this obscure subject be cleared up. (See Ann. de Chim. et Phys. ii. 217.)

Professor Berzelius terminates his paper by a bitter complaint against me for having made no use of the views which he suggested in his paper On the Cause of Chemical Proportions, in my Essay on the Theory of Definite Proportions in Chemical Combinations. He assures his readers that my view of the subject is radically bad, and that, by touching upon the theory, I have done much more injury than good to the doctrine of definite proportions. It would have been singular, indeed, if in a paper written and published (*Annals*, ii. 32, July, 1813) several months before his paper came into my possession (published *Annals*, ii. 443, December, 1813), I should have adopted views with which, at the time, I could not possibly be acquainted. If his views have not been adopted by the chemical world in general, the reason may be presumed to be that they have not been approved of. Some of them I have taken the trouble to examine and refute; and it may be presumed that my refutation was complete, as the points in dispute have been abandoned by Berzelius himself. Thus he now gives up his position that in the combinations of inorganic bodies one of the constituents always is present in the proportion of one atom; nor does he now contend for his whimsical assertion that if this opinion be rejected, the doctrine of definite proportions cannot be maintained. He has likewise given up his grand doctrine, which, in his opinion, constituted the basis of the whole theory of definite proportions; namely, that the oxygen in the acid of every salt is a multiple by a whole number of the quantity of oxygen in the base; for he allows that this does not hold in the phosphates nor the nitrates; and doubtless various other genera of salts will be found equally irreducible under it. Yet this law, demonstrated to be false, and admitted to be so by himself, is the only argument of any force, as far as I can perceive, which he has brought forward against the doctrine that *chlorine* is a simple substance.

With respect to my own view of the atomic theory, I admit that it is founded upon a supposition which is in a certain sense arbitrary, namely, that water is composed of 1 atom oxygen + 1 atom hydrogen. I have given my reasons for adopting that conclusion, and I still think that these reasons have weight. But in a practical point of view the only part of the doctrine of definite proportions which is of great importance is the determination of the numbers which represent the proportions in which bodies enter into compounds, numbers which Dr. Wollaston has denominated *equivalents*. It would be better, I think, in the first place, to be satisfied with these numbers. We shall be able to advance a step further when we are accurately acquainted with the composition of all the salts, and not till then. From Berzelius's analysis of the phosphates, we can readily ascertain the equivalent number for phosphoric acid. The equivalents for the bases in the salts which he analysed are as follows:—

Barytes	9·75
Yellow oxide of lead	14·00
Oxide of silver	14·75
Soda	3·925
Lime	3·625

Now the phosphates of these bases were found composed as follows:—

Phosphate of Barytes.

Acid	100	4·54
Base	214·46	9·75

Phosphate of Soda.

Acid	100	4·51
Base	87	3·925

Phosphate of Lead.

Acid	100	4·46
Oxide	314	14·00

Phosphate of Lime.

Acid	100	4·28
Lime	84·53	3·625
	80	4·53*

Phosphate of Silver.

$$\left.\begin{array}{lll}\text{Acid} \ldots\ldots\ldots & 100{\cdot}00 \ \ldots\ldots & 3{\cdot}13 \\ \text{Base} \ldots\ldots\ldots & 474{\cdot}16 \ \ldots\ldots & 14{\cdot}75\end{array}\right\}\text{or}\left\{\begin{array}{l}4{\cdot}5 \\ 14{\cdot}75 + \frac{14{\cdot}75}{2}\end{array}\right.$$

It is quite clear from these analyses that the equivalent number for phosphoric acid is 4·5.

But according to Berzelius, phosphoric acid is a compound of 1 atom phosphorus + 5 atoms oxygen, and it is composed of 100 phosphorus + 126·94 oxygen. According to this estimate, an atom of phosphorus weighs 3·93, and an atom of phosphoric acid 8·93—a number quite inconsistent with the preceding equivalent, and undoubtedly wrong. My number 3·5 is likewise wrong, but the error is much less.

Berzelius has likewise analysed two of the phosphites. Let us see what equivalent number will result from these analyses for phosphorous acid. He found these salts composed as follows:—

Phosphite of Barytes.

Acid	100	3·52
Barytes	276·59	9·75

Phosphite of Lead.

Acid	100	3·45
Base	405·48	14

Is it not obvious that 3·5 is the equivalent number for phosphorous acid? This is the number which I gave in my paper on phosphureted hydrogen as the equivalent for phosphoric acid. I am inclined at present to suspect that what I took for phosphoric acid was in reality *phosphorous acid*, and that my phosphorous acid was the hypophosphorous acid of Dulong. I shall verify this suspicion as soon as I have as much leisure as will enable me to undertake the requisite experiments. Were the supposition well founded, the composition of the three acids would be as follows:—

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* By my analysis.

	Phosphorus.	Oxygen.
Hypophosphorous acid....	1·5 +	1
	100 +	66·66
Phosphorous acid........	1·5 +	2
	100 +	133·33
Phosphoric acid	1·5 +	3
	100 +	200

If this opinion be true, we are far, indeed, from a true knowledge of the composition of phosphoric acid. At all events it will, I think, be acknowledged, even by Berzelius himself, that my mode of analysis or synthisis by the combustion of phosphureted hydrogen is susceptible of much greater precision than the very suspicious method had recourse to by Berzelius; and that the composition of phosphoric and phosphorous acids as determined by him is radically wrong. When he peruses what I have here stated, I dare say he will feel ashamed of the terms in which he has spoken of my determinations, and allow that a man may fall into mistakes, when treating of so difficult a subject, without any culpable carelessness or want of precision.

4. *Action of Phosphoric Acid on Indigo.*—Dobereiner tried the effect of phosphoric acid upon indigo, both when anhydrous, and when in the state of a hydrate. But in neither case was there any action whatever. Sulphuric acid, then, is the only known acid that has the property of dissolving indigo without destroying the colour. When the sulphate of indigo is boiled, a portion of the indigo precipitates, and the rest loses its blue colour, and is of course decomposed. (Schweigger's Journal, xiv. 372.)

5. *Uric Acid.*—Gay-Lussac has ascertained, by heating a mixture of uric acid and oxide of copper, and receiving the gaseous products over mercury, that this acid, when decomposed, yields two volumes of carbonic acid and one volume of azote. (Ann. de Chim. xcvi. 53.) It contains, therefore, two atoms of carbon and one atom of azote. In this respect it agrees with cyanogen. Probably it will be found to differ from hydro-cyanic acid by containing an atom of oxygen instead of hydrogen. It is likely that uric acid will be found to be composed of one atom oxygen, one atom azote, and two atoms carbon.

6. *Rosacic Acid.*—Vogel has made some experiments on rosacic acid, which is known to appear in urine in certain cases of disease. It is soluble in boiling alcohol, and by this means may be separated from uric acid. Sulphuric acid dissolves it, and converts it into uric acid. It gives a permanent red colour to sulphurous acid. Nitric acid converts it equally into uric acid. (Ann. de Chim. xcvi. 306.) From these experiments it would seem that rosacic acid approaches very near the uric acid in its nature.

7. *Acid of Stick Lac.*—Dr. John has discovered a new acid in stick lac by the following process. The lac is pulverized, and washed in water till it gives no further colour to that liquid. The watery

solution is evaporated to dryness, and the dry mass digested in alcohol. The alcoholic solution is treated in the same manner, and the dry residue is digested in ether. The ether, when evaporated, leaves a syrupy mass of a light wine colour. This mass, being dissolved in alcohol, and the solution diluted with water, lets fall some resin. It consists now of the new acid, combined with a very little potash and lime. To obtain it in a state of purity, precipitate it with acetate of lead, and separate the oxide of lead by means of the requisite quantity of sulphuric acid. The acid thus prepared possesses the following properties:—

It may be crystallized. It has a light wine-yellow colour. Its taste is acid; and it dissolves in water, alcohol, and ether. It throws down lead and mercury white, but occasions no precipitate when poured into lime-water, nitrate of silver, or nitrate of barytes. It precipitates the oxides of iron white. Its combinations with lime, soda, and potash, are deliquescent, and soluble in alcohol. (Schweigger's Journal, xv. 110.)

8. *Quantity of Carbonic Acid in the Atmosphere.*—M. Theodore de Saussure has lately published the result of a number of experiments to determine the relative proportion of carbonic acid in the atmosphere during summer and winter. His method was to fill a large glass globe with the air to be examined, and to put into it a quantity of barytes-water. The carbonic acid in the air was determined by the quantity of carbonate of barytes formed. The following tables exhibit his results:—

In winter 10000 parts of air in volume gave

Jan. 31, 1809, Temp. 23°, 4·57 parts of carbonic acid.
2, 1811, 20·3, 4·66
7, 1812, 34, 5·14

Or the mean gives 4·79 parts of carbonic acid gas in 10000 measures of air.

10000 parts of the same air in weight contain 7·28 parts of carbonic acid gas.

In summer 10000 measures of air gave

Aug. 20, 1810, Temp. of 71·6°, 7·79 parts of carbonic acid gas.
July 27, 1811, 71·6, 6·47
15, 1815, 84·2, 7·13

Or the mean gives 7·13 parts of carbonic acid gas in 10000 measures of air.

10000 parts by weight of the same air contain 10·83 parts of carbonic acid gas. (Ann. de Chim. et Phys. ii. 199.)

XII. SALTS.

1. *Sulphate of Manganese.*—M. Brandenburg, apothecary at Polotzk, has published a set of experiments pointing out the method of preparing a pure sulphate of manganese from the common black oxide of manganese of commerce. I may just state his last process, which was perfectly successful. Four parts of finely pulverized

black oxide of manganese were mixed in a common phial with six parts of strong sulphuric acid. The phial was put into a crucible surrounded with sand, and exposed for an hour and a half to a dull red heat. The white mass thus formed was thrown into cold water, and, after being digested a sufficient time, the liquid was filtered. A colourless solution is thus obtained, which, being set aside in a warm place, yields regular crystals of pure sulphate of manganese. (Schweigger's Journal, xiv. 336.)

2. *Metalline Muriates.*—It is at present nearly the general opinion of chemists that when the chlorides of the different metals are dissolved in water, they are converted into muriates. M. Chevreul has pointed out some facts which strengthen that opinion. Though these facts are not new, yet I shall state them here, because they are likely to appear more striking when collected together than when considered in an insulated state. (1.) Proto-chloride of iron is white; but becomes green when dissolved in water, and crystallizes in polyhedrons of the same colour. (2.) Perchloride of iron forms an orange-brown solution, which crystallizes in yellow needles. (3.) Chloride of cobalt is grey; but when thrown into water, it forms a red solution, like the proto-sulphate, proto-nitrate, and prot-acetate of cobalt. (4.) Chloride of nickel is golden-yellow, but it forms a green solution in water, like the proto-sulphate, proto-nitrate, and prot-acetate of nickel. (5.) Perchloride of copper is yellow; but its concentrated solution is green, and it becomes blue when diluted, like the other solutions of peroxide of copper. (Ann. de Chim. xcv. 308.)

3. *Chloride of Alumina.*—One of the most beautiful parts of calico printing is the art of discharging the Turkey red dye from different parts of a piece of cloth, which may be either left white or printed afterwards at pleasure. This process is performed by means of chlorine. Chloride of lime is dissolved in water, and decomposed by means of sulphuric or muriatic acid. The liquid thus impregnated with chlorine is applied in a very ingenious way, of which a description has been given by Mr. Wilson in the *Annals*, viii. 125. I have been informed that this ingenious process originated in Glasgow; and I will take it as a favour if any of my readers will furnish me with a history of the discovery. Mr. Wilson has found that chloride of alumina answers the purpose of discharging the Turkey red dye fully as well as pure chlorine, while it has the advantage over it of not injuring the texture of the cloth, and of not annoying the workmen by its noxious smell. He prepares it by adding to a solution of chloride of lime, of the specific gravity 1·060, a solution of alum of the specific gravity 1·100, as long as any precipitate falls. The precipitate is to be separated, and the clear liquid kept for use in close vessels. (See *Annals*, viii. 127.)

4. *Chlorates.*—When my historical sketch of the progress of the sciences during 1815 appeared in the *Annals* for last January, M. Vauquelin's paper on the chlorates had not been all published; or at least the number of the Ann. de Chim. for August, 1815, had

not come to London. I shall, therefore, insert here the remaining chlorates described by him in the last part of his paper.

(1.) *Chlorate of Zinc.*—Chloric acid dissolves the carbonate of zinc with effervescence; but the acid is not easily saturated by the oxide. The solution is very astringent. It crystallizes in short octahedrons. It does not precipitate nitrate of silver, and is therefore a pure chlorate. Chloric acid dissolves zinc without the least effervescence. The solution is astringent, the salt is very soluble, and crystallizes with difficulty. When heated in a phial, it gives out a mixture of oxygen and chlorine, and a white mass remains, which is a mixture of chloride and subchloride of zinc. When the salt is heated upon charcoal, a detonation takes place. This salt precipitates nitrate of silver. It is not, therefore, a pure chlorate. Probably the zinc is oxidized at the expense of the chloric acid.

(2.) *Chlorate of Iron.*—Chloric acid dissolves iron with great rapidity without the evolution of any gas. Considerable heat is produced during the solution. The liquid has at first a green colour, and astringent taste; but it soon becomes red, without the access of air. At first it gives a green precipitate with the alkalies, and is scarcely tinged by gallic acid; but after it has acquired a red colour the alkalies produce a red precipitate; and infusion of nutgalls and prussiate of potash strike a green colour with it. When evaporated, it is converted into a gelatinous mass, like coagulated blood. When dried, it became transparent, and is still soluble in water. It does not fuse upon burning charcoal, like the last salt. When heated, it gives out chlorine, but no oxygen. It precipitates the nitrate of silver, and therefore is not a pure chlorate. Probably it is a compound of chlorine and oxide of iron.

(3.) *Chlorate of Silver.*—This salt is easily obtained by placing in contact chloric acid and oxide of silver newly precipitated, and still moist. The salt is neutral; and when sufficiently concentrated, crystallizes in four-sided prisms, terminated by oblique faces. It dissolves in 10 or 12 times its weight of water, at the mean temperature of the atmosphere. Its taste is similar to that of nitrate of silver, but weaker. When triturated with sulphur, it produces flame, and an intense heat. On red-hot charcoal it fuses with a strong flame, and leaves chloride of silver. M. Vauquelin ascertained that this salt may be formed by passing a current of chlorine over oxide of silver; but if the current is too long continued, the chlorate is again decomposed, and chloride of silver formed.

(4.) *Chlorate of Lead.*—When chloric acid is poured upon litharge in the state of a fine powder, a combination speedily takes place. The liquid is colourless, and has a sweet and astringent taste. This salt is neutral, and crystallizes in brilliant plates: 500 parts of litharge produce 740 of the dry salt. On red-hot charcoal it melts, giving out white fumes, and nothing remains but some small grains of metallic lead: 700 milligrammes of this salt strongly heated furnished 111 cubic centimetres of oxygen gas mixed with a little chlorine. This amounts to about one-fifth of the weight of the salt. Sulphuric acid and the alkalies throw down a white precipitate from the solution of this chlorate.

(5.) *Chlorate of Copper.*—Peroxide of copper dissolves readily in chloric acid. The solution is green, and always contains a slight excess of acid. It crystallizes with difficulty, because the salt is deliquescent. It melts on burning coals, producing a green flame. A paper dipped in the solution of this salt, and held near the fire, burns with a fine green flame at a temperature not so high as would be sufficient to burn the paper alone. (See Ann. de Chim. xcv. 113.)

5. *Phosphates.*—Berzelius has lately made numerous experiments on the analysis of the phosphates, in order to determine the composition of phosphoric acid. (Ann. de Chim. et Phys. ii. 151.) I shall here give the facts which he has ascertained.

(1.) *Phosphate of Barytes.*—Barytes and phosphoric acid unite in three portions, forming a neutral salt, and two salts with excess of acid. The neutral salt is obtained by mixing phosphate of ammonia with muriate of barytes. Berzelius analyzed it by dissolving it in nitric acid, and throwing down the barytes by means of sulphuric acid: 7·5 parts of the salt furnished 7·798 parts of sulphate of barytes. Hence it is composed of

Phosphoric acid	31·8	100·00
Barytes	68·2	214·46
	100·0	

Biphosphate of barytes was formed by dissolving the neutral phosphate in phosphoric acid, filtrating the liquid, and evaporating it slowly in a platinum capsule. Crystals gradually formed, which were separated, and dried upon blotting paper. This salt contains water of crystallization, and has a good deal of resemblance to crystallized muriate of barytes. Its taste is slightly acid; but in other respects similar to that of the muriate. When heated, it swells, and forms a porous mass, like *burned alum.* It is decomposed by water, which removes the excess of acid. Its constituents are—

Phosphoric acid	42·54	100·00
Barytes	46·46	107·11
Water	11·00	
	100·00	

Thus we see that it contains just twice as much acid as the neutral phosphate.

Sesquiphosphate of barytes (*phosphate acidule de baryte* of Berzelius) is obtained by pouring a solution of the preceding salt into alcohol. A bulky precipitate falls, which, when washed with alcohol, and dried, forms a light white powder. It was found composed of

Phosphoric acid	39·13	100·0
Barytes	60·87	155·5
	100·00	

By this analysis it contains 1½ times as much acid as the neutral salt.

(2.) *Phosphate of Lead.*—Oxide of lead and phosphoric acid unite, likewise, in three proportions, forming a neutral salt, a salt with excess of acid, and a salt with excess of base. The analysis of the neutral salt puzzled Berzelius for a considerable time. He had prepared it by mixing nitrate of lead and phosphate of ammonia. At last he found that a double salt had been formed by the union of a portion of nitrate of lead with the phosphate of lead. He succeeded at last in forming pure phosphate of lead by mixing a boiling hot solution of muriate of lead with phosphate of soda. This salt was decomposed by means of sulphuric acid: five parts of it produced 5·15 parts of sulphate of lead. Hence it is composed of

Phosphoric acid	24	100
Oxide of lead	76	314
	100	

Superphosphate of lead is obtained by pouring hot muriate of lead into biphosphate of soda. The precipitate still reddens litmus, after it has been thoroughly washed with water. Berzelius found it composed of

Phosphoric acid	30·269	100·0
Oxide of lead	69·731	230·6

So that it is nearly similar in its composition to the sesquiphosphate of barytes.

Subphosphate of lead was obtained by digesting phosphate of lead in caustic ammonia. According to Berzelius, it is composed of

Phosphoric acid	17·48	100
Oxide of lead	82·52	472

Now 472 = 314 × 1·5. Thus it appears that the oxide in the subphosphate is 1½ times as great as that in the neutral phosphate of lead.

(3.) *Phosphate of Silver.*—Berzelius was able only to form a *subphosphate* of this salt. When nitrate of silver and phosphate of soda, both neutral, are mixed together, the liquid becomes acid, while a yellow precipitate falls. This subphosphate, according to Berzelius, is composed of

Phosphoric acid	17·025	100·00
Oxide of silver	82·975	487·38

(4.) *Phosphate of Soda.*—This salt is never quite neutral, acting always slightly, either as an acid or alkali. According to the analysis of Berzelius, it is composed of

Phosphoric acid	20·33	100
Soda	17·67	87
Water	62·00	
	100·00	

A biphosphate of soda likewise exists; but Berzelius did not analyse it with precision.

(5.) *Phosphate of Lime.*—Berzelius has met with so many anomalies in his experiments on the combinations of phosphoric acid and lime, that he has been able to form no satisfactory conclusion. When a solution of phosphate of soda is poured into a solution of neutral muriate of lime, the liquid loses its neutrality, and acts as an acid on vegetable blues. A crystallized precipitate falls, which was composed of

Phosphoric acid	41·90	100·00
Lime	35·42	84·53
Water	22·68	
	100·00	

If muriate of lime be poured into phosphate of soda, and the precipitate be digested in the excess of phosphate of soda, a phosphate of lime is obtained possessed of very different properties. It is gelatinous, like alumina, and was found composed of

Phosphoric acid	45·57	100
Lime	48·73	107
Water	5·70	

It does not appear necessary to state the other experiments which he made on the phosphates of lime, as the results obtained are so anomalous that no other conclusion can be drawn from them than that most of the substances examined by Berzelius were rather mixtures than true chemical compounds. His observations on my unpublished experiments strike me as somewhat premature, as he cannot possibly be acquainted with the method which I followed. Indeed, had he even heard my paper read to the Royal Society last winter, he could have known but a very small part of the very numerous experiments which I made on the phosphates. This defect in the paper was one of the reasons that induced me to withdraw it; though my principal motive was some new views which had occurred to me after that paper was read, and which I conceived would enable me to add considerably to its accuracy. Berzelius's experiments will be very useful in that point of view, as they draw the attention to a variety of particulars which I had overlooked. I hope hereafter to be able to resume the subject with additional advantages. In the mean time I may just say that I still consider the constitution of phosphorous and phosphoric acids as given in my paper on phosphureted hydrogen gas as likely to be accurate.

6. *Phosphate of Alumina.*—When phosphoric acid exists in the mineral kingdom united to oxide of iron, the method of determining its quantity hitherto followed consists in fusing the mineral with potash, and then precipitating the phosphoric acid by means of lime. Vauquelin has lately pointed out an inaccuracy to which this method is liable. If the mineral contain at the same time phosphoric acid and alumina, both will be dissolved by the potash, and both thrown down by the lime; so that the quantity of acid as deduced from such an experiment will be erroneous. He proposes to correct this error by digesting the precipitate in potash, which will dissolve the alumina, and leave the phosphate of lime. Pure alumina, when newly precipitated, is transparent and gelatinous; while the phosphate of alumina is white and opake. But this change in the appearance of the alumina does not always indicate the presence of phosphoric acid. Silica and lime give it the same appearance. (Ann. de Chim. xcvi. 213.)

7. *Phosphites.*—Gay-Lussac has published an experiment to show that when a phosphite is heated it is converted into a neutral phosphate by the decomposition of the water which it contains, and that little or no phosphorus is disengaged. He prepared phosphorous acid by the slow combustion of phosphorus, and saturated it with potash. This salt was put into a retort, to the beak of which was attached a bent tube dipping into water. The salt was heated rapidly. Hydrogen gas was driven off containing very little phosphorus, for it had but a slight smell, and did not burn spontaneously when it came in contact with the atmosphere. (Ann. de Chim. et Phys. i. 212.) This is an experiment which I have often made, and it never succeeded with me exactly as described by Gay-Lussac. Davy some years ago ascertained that the gas driven off in this case was a peculiar compound of hydrogen and phosphorus, to which he gave the name of hydro-phosphoric gas. From my experiments on this gas, I find that it is composed of two atoms hydrogen and one atom phosphorus. It does not burn when it comes in contact with air, but is easily fired by electricity or heat, and phosphoric acid is always formed during its complete combustion. Its smell is not so strong as that of phosphureted hydrogen gas; but it is very remarkable, and easily recognised.

Berzelius, during his experiments on phosphorus, analysed two of the phosphites. I shall here state the results which he obtained. Phosphite of lead he found composed of

Phosphorous acid	19·16	100
Oxide of lead	77·69	405·48
Water	3·15	
	100·00	

Phosphite of barytes is composed of

Phosphorous acid	24·31	100
Barytes	67·24	276·59
Water	8·45	
	100·00	

When the phosphorous acid is converted into phosphoric acid, these salts still continue neutral. (See Ann. de Chim. et Phys. ii. 228.)

8. *Borates.*—Leopold Gmelin has published a set of experiments on several of the borates, which have been hitherto very much neglected by chemists. I shall give here the results which he obtained.

(1.) *Borate of Barytes.*—This salt may be obtained by pouring a solution of a borate into muriate or acetate of barytes. It must be well washed, in order to be tolerably pure. It is a white powder, nearly as soluble in water as sulphate of lime. It is more soluble in hot than in cold water. In a red heat it swells a little, and is converted into a greenish coloured vesicular mass: 11·921 parts of it dissolved in diluted muriatic acid, and precipitated by sulphuric acid, gave 9·951 parts of sulphate of barytes. Hence the salt is composed of

Boracic acid	5·387	100
Barytes	6·534	121·29

(2.) *Borax.*—This salt, according to the experiments of Gmelin, is composed of

Boracic acid	35·6	100
Soda	17·8	50
Water	46·6	
	100·0	

He considers this salt (contrary to the usual opinion of chemists) as a compound of 1 atom acid + 1 atom soda + 9 atoms water. On that supposition an atom of boracic acid would weigh twice as much as an atom of soda, or 7·875. The salt formed by adding boracic acid to a solution of borax till it ceases to possess alkaline properties contains, according to Gmelin, three times as much boracic acid as exists in borax. Borax he considers as a borate of soda, and this salt as a triborate of soda.

(3.) *Borate of Ammonia.*—This salt is readily formed by dissolving crystallized boracic acid in caustic ammonia. If the ammonia be concentrated, the salt crystallizes during the preparation, and it may always be obtained in regular crystals by evaporation. The crystals are four-sided, and sometimes six-sided prisms, terminated by four-sided pyramids. The salt is hard, not altered by exposure to air, and has a slight alkaline taste, and re-acts as an alkali upon vegetable blues. When its solution is heated, it gives out ammonia, and by long continued boiling almost the whole of the ammonia may be driven off. According to Gmelin's analysis, it is composed of

Boracic acid	63·4	100
Ammonia	5·9	9·30
Water	30·7	
	100·0	

He considers it as a triborate, or as composed of three atoms of boracic acid and one atom of ammonia combined with 10 atoms of water.

By mixing solutions of borax and sulphate of magnesia, and setting aside the mixture to undergo spontaneous crystallization, Gmelin obtained two remarkable double salts, consisting of a combination of borax and sulphate of magnesia in two different proportions. (See Schweigger's Journal, xv. 245.)

XIII. MINERAL WATERS.

1. *Mineral Water of Caversham, in Berkshire.*—An anonymous correspondent has published in the *Annals*, viii. 123, an analysis of a mineral water at Caversham, in Berkshire, hitherto overlooked by medical men. He finds the constituents as follows:—300 cubic inches contain, of gaseous contents,

Carbonic acid	33 cubic inches
Sulphureted hydrogen	4
	37

Of solid contents,

Carbonate of soda	10 grains
Muriate of soda	6
Carbonate of iron	18
Carbonate of lime	9
	43

This mineral water would probably be found useful as a stomachic. Perhaps none of the acidulous waters of Great Britain contains a greater proportion of carbonic acid gas.

2. *Tunbridge Wells Water.*—We are indebted to Dr. Scudamore for a new and accurate analysis of the celebrated mineral spring at Tunbridge Wells, of which a short account has been given in the *Annals*, viii. 149. A gallon of the water was found to contain

Of gaseous matter	13·3 cubic inches
Of saline matter	7·68 grains

The gaseous matter consisted of

Carbonic acid	8·05 cubic inches
Oxygen	0·50
Azote	4·75

The saline matter consisted of

Common salt	2·46 grains
Muriate of lime	0·39
Muriate of magnesia	0·29
Sulphate of lime	1·41
Carbonate of lime	0·27
Oxide of iron	2·29
Trace of manganese and insoluble matter	0·44
Loss	0·13
	7·68

3. *Sea Water.*—Dr. Murray, of Edinburgh, has lately made a very careful analysis of sea water, in order to judge how far his views respecting mineral waters in general (of which an account was given in our historical sketch for last year) are applicable to sea water. The result will be published in the next volume of the Transactions of the Royal Society of Edinburgh. During his experiments he discovered a new double salt, composed of sulphate of magnesia and Glauber's salt. From the observations of Mr. Heales, it appears that this salt has been long used in London as a cathartic. (See Phil. Mag. xlviii. 202.)

XIV. VEGETABLE BODIES.

1. *Beet Sugar.*—Margraaf ascertained many years ago that crystals of sugar might be extracted from the beet; and Achard, of Berlin, endeavoured to show, by experiments in the large way, that sugar might be extracted from the beet with profit. Little attention was paid to these experiments till Bonaparte began his chimerical project of subjecting all Europe under his dominion. Great Britain being the most formidable enemy that he had to encounter, he endeavoured to destroy her in the first place by putting an end to her commerce, upon which, in his opinion, her very existence depended. With this view he contrived what he called the European system, by which Great Britain was excluded, as far as his influence extended, from all the markets of Europe. His own subjects, in consequence, were prevented from being supplied as usual with the commodities of India and America, many of which had become necessaries of life in France, as well as in Great Britain. Sugar, among other articles, rose to an enormous price, and at some times could scarcely be procured on any terms whatever. This drew Bonaparte's attention to the old experiments of the Prussian chemists, and he encouraged the establishment of manufactories in France for extracting sugar from the beet. Many accordingly were erected; some of them even were successful; but as the price of this sugar was considerably higher than that of sugar from the West Indies, the restoration of Peace will probably destroy the whole of these manufactories, if it has not done so already. Chaptal, who was himself a successful maker of beet sugar, conceiving that it

was worth while to preserve the recollection of the methods followed in preparing beet sugar, has published a long treatise on the subject, in which he describes all the details at length, and endeavours to prove that even in the present circumstances of France these manufactories may in certain cases be persisted in with advantage. I shall endeavour in this place to lay a sketch of the process followed before my readers.

The beets are deprived of their heads and tails, scraped with a knife, and then reduced to a pulp by a kind of rasp driven by the hand. Good beet yields between 65 and 75 per cent. of its weight of juice. The juice is let into a large caldron, and heated to the temperature of between 104° and 122°. Then slacked quick-lime is thrown in to the amount of $2\frac{1}{2}$ grammes to the litre of juice. The heat is then raised till the juice is nearly boiling hot. The fire is then extinguished. A crust forms upon the surface of the juice, which is carefully skimmed off. A stop-cock placed a foot above the bottom of the caldron is then opened, and the juice allowed to flow into another boiler. Lastly, a stop-cock at the bottom of the caldron is opened, and the juice at the bottom is allowed to pass through a filter, and mixed in the boiler with the remaining juice. In this new boiler the juice is made to boil. As soon as this takes place, a quantity of sulphuric acid previously diluted with 20 times its weight of water, and amounting to about one tenth of the lime previously used, is mixed with the liquid. It is better rather to allow a slight excess of lime to remain in the liquid than to have any excess of sulphuric acid. Three per cent. of animal charcoal is likewise added in the state of an impalpable powder. The liquid is then drawn off clear into a smaller and deeper boiler, where the boiling is continued till the whole is sufficiently concentrated to allow the sugar to granulate. The raw sugar thus formed is refined in the usual way. (See Ann. de Chim. xcv. 253.)

The sugar thus formed has exactly the appearance and the chemical properties of sugar from the sugar-cane. It has likewise the same figure of crystals. Hence there can be no doubt that it is the very same substance.

2. *Albumen and Gluten compared.*—In Schweigger's Journal, xiv. 294, there is a paper by Mr. H. F. Link, in which he compares the chemical properties of *animal albumen* and the *gluten* of wheat. I have inserted a translation of this paper in the *Annals*, vii. 455, to which I refer the reader. It will be seen by a perusal of that paper that the two substances have a very close resemblance to each other, supposing always that the albumen has been previously coagulated by heat.

3. *Method of separating Gluten from Starch.*—Kirchhoff, to whom we are indebted for the discovery of the method of converting starch into sugar, observed that the process did not succeed so well with starch from grain as with potatoe starch. This he considered as owing to the presence of gluten, with which starch from

grain is always more or less contaminated. He fell upon the following method of separating this gluten, which succeeded perfectly: 3 lb. of potash are dissolved in 100 lb. of water, and the solution mixed with 4 lb. of good slacked quick-lime. The mixture is frequently agitated during three hours, and then the clear liquid is drawn off, and kept for use in close vessels. For every pound of starch to be purified, a pound of this alkaline ley must be taken. It must be poured on the starch, and allowed to remain in contact with it at a moderate temperature for two or three days. It acquires a brown colour from the gluten, which it dissolves, and the starch becomes much whiter and purer. (Schweigger's Journal, xiv. 385.)

4. *Malambo Bark.*—This is the bark of an unknown tree in South America, which has been long used in medicine, and is considered as an effectual cure of locked jaw. From the accounts of MM. Bonpland and Zea, there is reason to consider the plant as belonging to the family of *magnolia*, and very probably to the genus *wintera*. A chemical examination of this bark has been published, both by M. Cadet, in the Journal de Pharmacie, and by M. Vauquelin, in the Ann. de Chim. Its principal ingredients are three in number, namely,

(1.) *A volatile Oil*, obtained by distilling a mixture of one part of bark and 10 parts of water. It has a yellow colour, and a smell intermediate between that of pepper and thyme. Its taste is acrid. It is very soluble in alcohol, and a little so in water. It is specifically lighter than water.

(2.) *A Resin*, obtained by macerating the bark in alcohol, and evaporating that liquid. This resin is very abundant. Its colour is brown. It is brittle, and has the usual resinous fracture. When put into the mouth, it appears at first to have no flavour, but an intensely bitter taste gradually developes itself. It is very soluble in alcohol, and is again precipitated by the addition of water. It is insoluble in alkalies. When thrown upon a hot body, it is almost entirely dissipated in smoke, which has the odour of frankincense.

(3.) *An Extract*, which is obtained by macerating the bark in water. Its colour is yellowish brown. It is brittle when dry; but attracts moisture when exposed to the atmosphere. When properly washed in alcohol, it has no bitter taste. When heated in close vessels, it yields a brown oil, and a liquid which reddens the infusion of litmus; but from which potash disengages a sensible quantity of ammonia. The residual charcoal, when burnt, leaves a notable quantity of carbonate of potash, coloured green by manganese. (See Ann. de Chim. xcvi. 113.)

5. *Cork.*—Chevreul has published a very elaborate set of experiments on this substance, which, from its yielding suberic acid when treated with alcohol, is considered by chemists as consisting chiefly of a peculiar vegetable principle, to which the name of *suber* has been given. Chevreul has contrived a new apparatus for the analysis of vegetable bodies. It consists of a small Papin's digester,

having a vessel of silver adapted to its inside, and having a tube proceeding from its top connected with a set of Woulf's bottles, in order to collect the liquid products that come over. The vegetable substance is put into this digester first with water; and when that liquid has extracted every thing that is soluble, the same process is repeated with alcohol. The advantage of the apparatus is, that the heat of these liquids may be increased considerably above the boiling point, which enables them to exert a more powerful solvent energy. The analysis of cork was given as an example of this method.

Cork, when treated in this manner with water, gave out an *aromatic principle*, and a little *acetic acid*, which passed over with the water into the receiver. The *extract* formed by the water contained two *colouring matters*, the one *yellow*, the other *red*; an *acid*, the nature of which was not determined; *gallic acid*; an *astringent substance*; a *substance containing azote*; a *substance soluble in water, and insoluble in alcohol*; *gallate of iron*; *lime*; and traces of *magnesia*: 20 parts of cork thus treated by water left 17·15 of insoluble matter.

The undissolved residue, being treated a sufficient number of times with alcohol in the same apparatus, yielded a variety of bodies, but which seem reducible to three; namely, *cerin*, *resin*, and an *oil*.

Cerin is a name which Chevreul has thought proper to give a crystallized substance that precipitates gradually when the alcohol digested on the cork is concentrated to one sixth of its bulk, and then set aside. The name is unfortunate, as it had been already applied by John to that part of common wax which dissolves in alcohol. The insoluble part of wax he had denominated *myricine*. If, therefore, the new substance of Chevreul be different from the cerin of John, as would appear to be the case from his experiments, it will be necessary to contrive a new name for it. Cerin is white, and in small needles. It does not melt in boiling water, but becomes soft, and sinks to the bottom of that liquid; while wax melts at 145°, and swims upon the surface of water. When heated or distilled, it undergoes nearly the same changes as wax. It is rather more soluble in alcohol than wax; 1000 parts of boiling alcohol dissolving, according to Chevreul, 2·42 parts of cerin and only two parts of wax. Nitric acid gradually dissolves it, and converts a portion of it into oxalic acid. It did not dissolve in an alcoholic solution of potash.

The 20 parts of cork thus treated with alcohol and water still weighed 14 parts. They consisted of suber, but not in a state of complete purity. (Ann. de Chim. xcvi. 141.)

6. *Action of Tannin on Mucilage.*—Grassmann has published a set of experiments showing that when infusion of nutgalls, or any other liquid containing tannin, is mixed with a vegetable mucilage, a precipitate falls, which he calls *tannate of mucilage*. The muci-

lages on which the experiments were made were syrup of marsh mallows, Iceland lichen, lintseed, and quinceseeds. (Schweigger's Journal, xv. 42.)

I ascertained long ago that gum is not precipitated from water by tannin. Dr. Bostock found that solutions of pure mucilage were not altered by infusion of nutgalls. I conceive, therefore, that the juices examined by Grassmann contained starch, and that the precipitate which he obtained was a *tannate of starch*, or rather perhaps a *tannate of gluten*, to which its properties seem to make it approach.

7. *Malting.*—It is well known that during the process of malting a sweet matter is generated in grain. When barley meal is infused in hot water, and kept in that state for some time, the same saccharine matter, as is well known, is formed. No light has hitherto been thrown upon this process, though it is essential towards the theory of brewing and distillation. But Kirchhoff, whose views were naturally turned towards this subject, by his discovery of the method of converting starch into sugar by means of acids, has lately published an experiment, which constitutes an essential and important step in the theory of fermentation. Barley meal contains both gluten and starch. If pure starch be infused in hot water, it is not converted into sugar. Neither does gluten become saccharine matter when treated in the same way. But if a mixture of pure dried pulverized wheat gluten and potatoe starch be infused in hot water, the starch is converted into sugar. During the process an acid is evolved; yet the gluten is little altered; and if the liquid be filtered, most of it remains upon the filter. But it does not answer when employed a second time to convert starch into sugar. It appears, then, that it is the gluten which acts upon the starch, and converts it into sugar. By melting, the gluten undergoes a change, which enables it to act more powerfully in turning the starch of raw grain into sugar. (Schweigger's Journal, xiv. 389.)

8. *Clarifying the Syrup of the Sugar Cane*—In the Ann. de Chim. xcv. 232, it is stated that a Frenchman, M. Dorion, has pointed out a very simple mode of clarifying the syrup of the sugar-cane. He merely throws into the boiling juice a certain quantity of the bark of the *pyramidal ash* in powder. The sugar planters of Guadaloupe, it is stated, have made him a present of a hundred thousand francs; those of Martinique have given him an equal sum; and the English planters have purchased the secret at the expense of four hundred thousand francs.

I have had some conversation on the subject of this statement with a friend of mine, a sugar planter lately come from the West Indies. He informs me that this plan has been tried in the West Indies, that it has been known for years, that he himself has employed it; but he never heard M. Dorion's name mentioned by any person, and is quite sure that the alleged purchase of the method by the English planters is not true.

XV. ANIMAL BODIES.

1. *Membranes.*—Dr. John has published a chemical analysis of the membranes of different parts of the body, of which I inserted a translation in the *Annals*, vii. 419. The results were as follows:—

(1.) *Epidermis* of the foot. It was composed of

Indurated albumen	93 to 95
Mucus, with a trace of animal matter	5
Lactic acid	1
Lactate of potash	
Phosphate of potash	
Muriate of potash	
Sulphate of lime	
Ammoniacal salt	
Phosphate of lime	
Manganese? and iron	
Soft fat	0·05

(2.) *Epidermis from the Arm of a Woman afflicted with Herpes* was composed of

Indurated albumen	92 to 93
Mucus, becoming insoluble by evaporation, and gelatinous mucus, precipitated by nutgalls	6 to 7
Lactic acid, and the above stated salts	1
Soft fat	$\frac{3}{4}$ to 1

(3.) *Horns of Black Cattle* were composed of

Indurated albumen	90
Gelatinous albumen with osmazom?	8
Lactic acid	1
Lactate of potash	
Sulphate, muriate, and phosphate of potash	
Phosphate of lime	
Trace of oxide of iron	
Ammoniacal salt	
Fat	1

(4.) *Hoofs of Horses* possess all the characters of horn.

2. *Sugar of Diabetes.*—Chevreul, by concentrating diabetic urine, and setting it aside, obtained the sugar in small crystals. These being dissolved in boiling alcohol, the liquid deposited white crystals of the sugar. It possessed all the properties of the sugar of grapes. Its crystals have the same shape; it is equally soluble in water and alcohol with that sugar; and, like it, melts when exposed to a gentle heat. (Ann. de Chim. xcv. 319.)

3. *Colouring Matter of the Blood*—Some years ago Mr. Brande published an important paper on the blood, in which he demonstrated, by decisive experiments, that the colouring matter of that fluid was not subphosphate of iron, as had hitherto been supposed,

but a peculiar animal matter, the properties of which he describes. A similar doctrine had been maintained by Berzelius; but his work on Animal Chemistry, having been published in the Swedish language, remained unknown to the chemical world in general. M. Vauquelin has recently verified the experiments of Mr. Brande, and added some new ones on the same subject. According to his experiments, the colouring matter of the blood may be obtained in the following manner:—

Let the clot or coagulum of blood, well freed from the serum, be put upon a seirce, and well mixed with four parts of sulphuric acid diluted with eight parts of water. Heat the mixture to the temperature of 158°, and keep it at that temperature for five or six hours. Filter the liquid while hot, and wash the residue with as much hot water as you employed of acid. Evaporate the liquids to one-half, and then add ammonia till the excess of acid is almost, but not quite, saturated. The colouring matter precipitates. Decant off the clear liquid, and wash the colouring matter with water till that liquid ceases to precipitate the nitrate of barytes. Then throw it upon a filter; and when well drained, scrape it off with an ivory knife, and let it dry in a capsule. Thus prepared, it possesses the following properties:—

It has neither taste nor smell. When suspended in water, it has a wine-red colour; but when dry, it appears as black as jet. In this state it dissolves readily both in acids and alkalies, and gives a purple colour to the solutions. Muriate of barytes does not occasion any precipitate in its solution in muriatic acid. Nor is any change produced by gallic acid or prussiate of potash. The infusion of nutgalls precipitates it, but does not occasion any change of colour. When heated, it neither alters its form nor colour, but gives out an animal odour, and furnishes carbonate of ammonia and a purple oil, but scarcely any gas. The charry residuum is as bulky as the original substance. Diluted nitric acid dissolves it without altering its colour. Nitrate of silver does not render the solution turbid; but acetate of lead throws down a brown precipitate. (Ann. de Chim. et Phys. i. 9.)

4. *Cyst in the Human Liver.*—Laugier has examined the matter contained in a cyst attached to the liver of a woman between the age of 60 and 70. When treated with boiling alcohol, that liquid, on cooling, deposited crystals of adipocire. The residue was dry. When triturated with potash, it gave out very little ammonia, and did not sensibly dissolve. When burned, it left 78 per cent. of its original weight, which turned out to be phosphate of lime. (Ann. de Chim. et Phys. ii. 126.)

5. *Chyme.*—We are indebted to Dr. Marcet for the examination of a quantity of chyme from the stomach of a turkey. It was a homogeneous, brownish, opake pulp, having the smell which is peculiar to poultry. It was neither acid nor alkaline, and became putrid in 12 days. When evaporated to dryness, it left nearly one fifth of its weight of solid matter. It contained albumen. When

burned, it left 12 parts in the 1000 of charcoal. This residuum contained iron, lime, and an alkaline muriate. (See *Annals of Philosophy*, vii. 235.)

6. *Chyle.*—We are indebted to the same chemist for an examination of chyle collected from the thoracic ducts of dogs within three hours after they had been fed. Chyle from vegetable food was a semitransparent, inodorous, colourless fluid, having a very slight milky hue. It contained a coagulum like the white of an egg, but pink. This coagulum contained albumen. The specific gravity of the serous portion was 1·02. It did not putrefy in 10 days, but acquired a smell similar to that of sour cream. It contained abundance of albumen. When evaporated, it left 4·8 per cent. of a yellow, deliquescent residue.

The chyle from animal food was white and opake, and had a more distinct pink hue. In other respects it was similar to the preceding. Only the coagulum putrefied sooner, and the solid residue of the serum was seven per cent. These liquids, when distilled, gave out moisture, carbonate of ammonia, and a heavy fixed oil. The vegetable serum left three per cent. of charcoal, the animal serum only one per cent. The presence of iron was recognised in the residuum, and the same proportion of salts that exist in animal fluids in general. (See *Annals*, vii. 234.)

7. *Excrements of Silk Worms.*—Brugnatelli has discovered the existence of uric acid in the red excrementitious matter emitted by the phalenas of silk worms. (Ann. de Chim. xcvi. 55.)

8. *Gases in the Intestines of a healthy Man.*—MM. Magendie and Chevreul lately examined the gaseous bodies in the intestines of four healthy young men, who were executed in Paris, about an hour after the execution. The results were as follows:—

In the stomach, the gases consisted of

Oxygen	11·00
Carbonic acid	14·00
Hydrogen	3·55
Azotic	71·45
	100·00

In the small intestines, of

Oxygen	0·00
Carbonic acid	24·39
Hydrogen	55·53
Azotic	20·08
	100·00

In the large intestines, of

Oxygen	0·00
Carbonic acid	43·50
Carbureted hydrogen, with traces of sulphureted hydrogen	5·47
Azotic	51·03
	100·00

In another individual, the gases of the small intestines were—

Oxygen	0·00
Carbonic acid	40·00
Hydrogen	51·15
Azote	8·85
	100·00

Those in the large intestines, of

Oxygen	0·00
Carbonic acid	70·00
Hydrogen and carbureted hydrogen	11·60
Azotic	18·40
	100·00

The stomach in this individual contained only a single bubble of gas.

In another individual, the gases in the small intestines were

Oxygen	0·00
Carbonic acid	25·00
Hydrogen	8·40
Azotic	66·60
	100·00

The cœcum contained

Oxygen	0·00
Carbonic acid	12·50
Hydrogen	7·50
Carbureted hydrogen	12·50
Azotic	67·50
	100·00

I should like to have seen the method stated which was employed in analysing this gas.

The rectum contained

Oxygen	0·00
Carbonic acid	42·86
Carbureted hydrogen	11·18
Azotic	45·96
	100·00

(See Ann. de Chim. et Phys. ii. 292.)

VII. MINERALOGY.

This branch of science, being a dependant upon chemistry, shares with it the general attention, and makes, in consequence, more rapid advances than most of the other physical sciences. We shall, as usual, divide it into the two great departments of *Oryctognosy* and *Geognosy*.

I. ORYCTOGNOSY.

This branch of mineralogy, as far at least as the analyses of minerals are concerned, owes more to Professor Berzelius during the last two or three years than to any one else. But there are some improvements applying to mineralogy in general which we must first notice before we state the analyses that have made their appearance since our last historical sketch.

1. *New Blowpipe.* — The new blow-pipe, invented by Mr. Brooke, and executed by Mr. Newman, of which a description has been given in the *Annals*, vii. 367, promises to constitute one of the greatest improvements which has been hitherto introduced into practical mineralogy. I have no doubt that it will speedily come into general use, and that it will enable the student of mineralogy to determine many important particulars which he has hitherto been too often obliged to take upon trust. Dr. Clarke has already shown what an important instrument of analysis this blow-pipe may be made when a proper mixture of oxygen and hydrogen gases is substituted for common air.

2. *Electric Properties of some Minerals.*—Haüy has observed that some varieties of electric calamine, or silicated zinc, are constantly electric at the common temperature of the air, and do not require to be heated. This is the only mineral hitherto observed to possess that property. But some Spanish tourmalines become electric by simple pressure between the hands. Some topazes, especially those of Siberia, of a white colour, preserve the electric virtue for a very long time after having been heated. (Ann. de Chim. et Phys. i. 447.)

3. *Pure chemical mineral System.*—Berzelius has published, in the fourth volume of Afhandlingar i fysik, kemi och Mineralogi, p. 1, printed at Stockholm towards the end of 1815, an elaborate paper, entitled, *Försök till ett rent kemiskt mineral System, Essay towards a pure chemical System of Mineralogy.* In this paper he takes a review of some of the most recent mineral systems, namely, those of Werner, Haussman, and Haüy, and points out their defects and inconsistency with his usual freedom. He then gives us his own arrangement of the mineral kingdom, founded upon the view which he had already explained in his Attempt to establish a pure scientific System of Mineralogy, an English translation of which was published in London in 1814. It was my intention to have entered in this place into considerable details respecting Berzelius's views; and the more so, because I perceive, with regret, that his intentions have been misunderstood, and that his opinions have been treated, in a cotemporary journal, with a harshness and want of respect to which they are by no means entitled. But this historical sketch has already extended to such a length, that I can do no more than exhibit an imperfect table of his classification. It will serve, at least, to give the reader an idea of the principles of his classification.

CLASS I.

It consists of substances formed according to the principles of unorganic nature, that is, in which the compound bodies of the first order contain only two elements.

A. OXYGEN.

	Oxygen	O

B. COMBUSTIBLE BODIES.

Order I. Metalloids.

First Family: Sulphur.

Common	Sulphur	S
Oxides	Sulphurous acid	$\ddot{S}$
	Sulphuric acid	$\dddot{S}$

Second Family: Muriaticum.

Oxides	Muriatic acid	$\ddot{M}$

Third Family: Nitricum.

Suboxide	Azote	$\dot{N}$

Fourth Family: Boron.

Oxide	Boracic acid	$\ddot{B}$

Fifth Family: Carbon.

Common	Diamond Anthracite	C
Oxide	Carbonic acid	$\dddot{C}$

Sixth Family: Hydrogen.

Sulphuret	Sulphur. hydrogen	$2H + S$
Carburet	Carbur. hydrogen	$2H + C$
Oxide	Water	$2H + O$

Order II. Electro-negative Metals.

It includes those metals whose oxides in combination with other bodies rather perform the office of acids than of bases.

First Family: Arsenic.

Native	Native arsenic	As
Sulphurets	Realgar Orpiment	
Oxide	Arsenic flowers	$\ddddot{\mathrm{As}}$

Second Family: Chromium.

Oxide	Chromocre	$\dddot{\mathrm{Ch}}$?

Third Family: Molybdenum.

Sulphuret	Molybdena	$Mo + 2S$
Oxide	Molybdenum ochre	$\dddot{Mo}$

Fourth Family: Antimony.

Native	Native antimony	Sb
Sulphurets	Sulphuret	$Sb + 3S$
	Plumose ore ?	
	Tinder ore ?	
	Red antimony ore	$\dddot{Sb} + 2SbS^3$
Oxides	Radiated oxide of antimony	$\ddddot{Sb}$
	Antimony ochre	$\overset{:::}{Sb}$?

Fifth Family: Titanium.

Oxides	Anatase
	Ruthil

Sixth Family: Silicon.

Oxide	1. Pure	Rock Crystal	$\dddot{Si}$
		Quartz	
		Calcedony, &c.	
	2. Mixed	Carnelian	
		Agate	
		Jasper	
		Iron flint, &c.	

Order III. Electro-positive Metals.

Those metals whose oxides rather perform the office of bases than of acids.

DIVISION I.—*Metals whose oxides at a higher temperature, either alone, or by the intervention of charcoal powder, are reduced, and constitute the radical of the substances formerly called metallic oxides.*

First Family: Iridium.

Osmiet	Native iridium	$I + Os$?

Second Family: Platinum.

Native	Platina sand	Pt
	Black platina	

Third Family: Gold.

Native	Native gold	Au
Telluret	Graphic ore	$AgT^2 + 3AuT^6$
	Yellow ore	$AgT^2 + 2PbT^2 + 3AuT^3$

Fourth Family: Mercury.

Native	Native mercury	Hg
Sulphuret	Cinnabar	HgS^2
	Liver ore	
	Stinkzinober	
Muriates	Mercurial horn ore	$\ddot{Hg} + 2\ddot{M}$
	Native calomel	$\ddot{Hg} + \ddot{M}$

Fifth Family: Palladium.

Native	Palladium	Pa

Sixth Family: Silver.

Native	Native silver	Ag
Sulphurets	Silver glance	$Ag\,S^2$
	Brittle glance ore	
	Red silver ore	$\dddot{Sb} + 2\,Sb\,S^3 + 6\,Ag\,S^2$
Stibiets	Antimonial silver ore	$Ag^2\,Sb$
	Silver antimonial ore	$Ag^3\,Sb$
Aurets	Electrum	$Ag\,Au^2$
	Auriferous silver	$Ag^2\,Au$
Hydrargyret	Solid amalgam	$Ag\,Hg^2$
	Liquid amalgam	
Muriate	Horn silver	$\ddot{Ag}\,\ddot{M}^2$
Carbonate	Grey silver ore	$\ddot{Ag}\,\ddot{C}^2 + \ddot{Ag}\,\ddddot{Sb}$?

Seventh Family: Bismuth.

Native	Native bismuth	Bi
Sulphurets	Bismuth glance	$Bi\,S^2$
	Subsulphuret	$Bi^2\,S$
	Needle ore	$Pb\,S^2 + 2\,Cu\,S + 2\,Bi\,S^2$
Oxide	Bismuth ochre	$\ddot{Bi}$

Eighth Family: Tin.

Sulphuret	See Copper	
Oxide	Tin-stone	$\ddddot{Sn}$
	Wood tin	

Ninth Family: Lead.

Native	Native lead	Pb
Sulphurets	Galena	$Pb\,S^2$
	Containing silver	
	Containing cobalt, &c.	
	Antimonial lead ore	$Pb\,S^2 + 2\,Cu\,S + Sb\,S^3$
	Light white silver ore	$Sb\,S^3 + Ag\,S^2 + 5\,Pb\,S^2$
	Dark white silver ore	$Ag\,S^2 + 4\,Sb\,S^3 + 5\,Pb\,S^2$
	Compact galena ?	
	Bismuth silver ore	$Ag\,S^2 + 2\,Pb\,S^2 + 3\,Bi\,S^2$
Telluret	Black tellurium ore	$Au\,Te^3 + Pb\,Te^2 + 2\,Pb\,S^2$
Oxides	Yellow oxide	$\ddot{Pb}$
	Native minium	$\dddot{Pb}$
Sulphate	Sulphate of lead	$\ddot{Pb}\,\dddot{S}^2$
Murio-carbonate	Corneous ore	$\ddot{Pb}\,\ddot{M}^2 + \ddot{Pb}\,\ddot{C}$
Phosphate	Green lead ore	$\ddot{Pb}\,\ddot{P}^2$
	Fibrous and conchoidal phosphate	
Carbonate	White lead ore	$\ddot{Pb}\,\ddot{C}^2$
	Black lead ore	
Chromate	Red lead ore	$\ddot{Pb}\,\dddot{Ch}$
Molybdate	Yellow lead ore	$\ddot{Pb}\,\dddot{Mo}^2$

Tenth Family: Copper.

Native	Native copper	Cu
Sulphurets	Grey copper ore	$Cu\ S$
	— from Rudolstadt	$Fe\ S^2 + Cu\ S$
	— from Westanfors Eriksgrufva	$Fe\ S^2 + 4\ Cu\ S$
	— from Hittedal	$Fe\ S^2 + 8\ Cu\ S$
	Black copper ore	
	Schwarzgultigerz	
	Lead fahlore	$(Pb\ Sb) + 2\ Cu\ S + 2\ Fe\ S^2$
	Copper fahlore	
	Tin pyrites	$Sn\ S^2 + 2\ Cu\ S$
	Copper bismuth ore	$Bi\ S^2 + 2\ Cu\ S$
Oxides	Red copper ore	$\dot{Cu}$
	Copper black	$\ddot{Cu}$
Sulphate	Sulphate of copper	$\ddot{Cu}\ \dddot{S}^2 + 10\ H^2\ O$
	Green schlag of grey copper ore	$\ddot{Cu}\ 1\frac{1}{2}\ \dddot{S} + 3\ H^2\ O$
Submuriate	Copper sand	$\ddot{Cu}^4\ \ddot{M} + 8\ H^2\ O$
Subphosphate	Phosphate of copper	$\ddot{Cu}\ \ddot{P}$
Carbonate	Malachite	$\ddot{Cu}\ \ddot{C} + H^2\ O$
Hydro-carbonate	Azure copper ore	$\ddot{Cu} + 2\ H^2O + 2\ddot{Cu}\ \ddot{C}$
	Copper green	
Arseniate	Trihedral oliven ore	$\ddot{Cu}\ 1\frac{1}{2}\ \overset{:::}{As}$
	Arseniate of copper	$\ddot{Cn}\ 1\frac{1}{2}\ \overset{:::}{As} + 3\ H^2\ O$
Subarseniate	Bournon's arseniate (2d, 3d, and 5th variety)	$\ddot{Cu}^3\ \overset{:::}{As} + 12\ H^2 O$
	Lenticular copper ore	$\ddot{Cu}^6\ \overset{:::}{As} + 36\ H^2 O$
Siliciates	Dioptase	
	Silicious copper ore	$\ddot{Cu}\ 1\frac{1}{2}\ \dddot{S}^3 + 6\ H^2\ O$

Eleventh Family: Nickel.

Arseniet	Copper nickel	$Ni\ As$?
Oxide	Black ore of nickel	$\ddot{Ni}$
Arseniate	Nickel bloom	
Siliciate	Pimelite	$\ddot{Ni}\ \dddot{S}^4 + 20\ H^2\ O$

Twelfth Family: Cobalt.

Sulphuret	Cobalt pyrites	$Fe\ S^4 + 4\ Cu\ S + 12\ Co\ S^3$
Arseniets	Glance cobalt	$Co\ As$
	Grey cobalt ore	$Co\ As + Fe\ As$
	White cobalt ore	$Fe\ As^2 + 3\ Co\ As^2 (+ 2\ Fe\ S^4)$
Oxide	Black cobalt ore	$\dddot{Co}$
Sulphate	Sulphate of cobalt	
Arseniates	Cobalt bloom	$\ddot{Co}\ 1\frac{1}{2}\ \overset{:::}{As} + 6\ H^2\ O$
	Cobalt ochre	

Thirteenth Family: Uranium.

Oxides	Pitch ore	$\ddot{U}$

	Uranmica	$\ddot{\mathrm{U}} + 2\,\dddot{\mathrm{U}}$
	Uran ochre	$\dddot{\mathrm{U}}$

Fourteenth Family: Zinc.

Sulphuret	Blende	$\mathrm{Zi}\,\mathrm{S}^2$
Oxide	Zinc ochre	$\ddot{\mathrm{Zi}}$
Sulphate	White vitriol	$\ddot{\mathrm{Zi}}\,\dddot{\mathrm{S}}^2 + 10\,\mathrm{H}^2\mathrm{O}$
Carbonates	Sparry calamine	$\ddot{\mathrm{Zi}}\,\ddot{\mathrm{C}}^2$
	Earthy calamine	$\ddot{\mathrm{Zi}}\,\ddot{\mathrm{C}} + 2\,\mathrm{H}^2\mathrm{O}$
Siliciate	Silicious calamine	$\ddot{\mathrm{Zi}}\ 1\frac{1}{2}\ \dddot{\mathrm{Si}}$
Aluminate?	Gahnite	$\ddot{\mathrm{Zi}}\ \dddot{\mathrm{Al}}^4$

Fifteenth Family: Iron.

Native	Fossil iron Meteoric	Fe
Sulphuret	Magnetic pyrites	$\mathrm{Fe}\,\mathrm{S}^4 + 6\,\mathrm{Fe}\,\mathrm{Si}^2$
	Light magnetic pyrites	$\mathrm{Fe}\,\mathrm{S}^4 + 2\,\mathrm{Fc}\,\mathrm{Si}^3$
	Iron pyrites	$\mathrm{Fe}\,\mathrm{S}^4$
Carburet	Graphite	$\mathrm{Fe}\,\mathrm{C}^{200}$?
Arsenite	Mispickel, mixed with pyrites	$\mathrm{Fe}\,\mathrm{As}^2 + \mathrm{Fc}\,\mathrm{S}^4$
Telluret	Native tellurium	$\mathrm{Fe}\,\mathrm{Te}^{10}$?
Oxides	Hematite	$\dddot{\mathrm{Fe}}$
	Magnetic iron ore Fer oligiste	$\ddot{\mathrm{Fe}} + 2\,\dddot{\mathrm{Fe}}$
Sulphates	Green vitriol	$\ddot{\mathrm{Fe}}\,\dddot{\mathrm{S}}^2 + 14\,\mathrm{H}^2\mathrm{O}$
	Red vitriol	$\ddot{\mathrm{Fe}}\,1\frac{1}{2}\,\dddot{\mathrm{S}}^2 + 3\,\dddot{\mathrm{Fe}}\,\dddot{\mathrm{S}}^2 + 36\,\mathrm{H}^2\mathrm{O}$
	Atramentstein	$\dddot{\mathrm{Fe}}^2\,\dddot{\mathrm{S}} + 6\,\mathrm{H}^2\mathrm{O}$
	Pitch-iron ore	$\dddot{\mathrm{Fe}}^4\,\dddot{\mathrm{S}} + 12\,\mathrm{H}^2\mathrm{O}$
Phosphate	Blue iron earth Native Prussian blue	$\ddot{\mathrm{Fe}}\,\ddot{\mathrm{P}}^2 + 4\,\mathrm{H}^2\mathrm{O}$
Carbonate	Sparry iron ore	$\ddot{\mathrm{Fe}}\,\ddot{\mathrm{C}}^2$
Arseniate	Cube ore	
	Strahlenerz	$\ddot{\mathrm{Cu}}^3\,\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{As}} + 2\,\dddot{\mathrm{Fe}}^2\,\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{As}} + 6\,\mathrm{H}^2\mathrm{O}$
	Flockenerz	$\ddot{\mathrm{Pl}}\,1\frac{1}{2}\,\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{As}} + 2\,\dddot{\mathrm{Fe}}^2\,\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{As}} + 12\,\mathrm{H}^2\mathrm{O}$
Chromite	Chromate of iron	$\dddot{\mathrm{Al}}\,\dddot{\mathrm{Ch}} + 2\,\dddot{\mathrm{Fe}}\,\dddot{\mathrm{Ch}}$?
Titaniates	Menachan Nigrin Compact magnetic iron-stone	
Siliciate	Hedenbergite	$\ddot{\mathrm{Fe}}\,\dddot{\mathrm{Si}}^2 + 4\,\mathrm{H}^2\mathrm{O}$
Hydrate	Limonite Moorerze	

Sixteenth Family: Manganese.

Sulphuret	Sulphuret of manganese	
Superoxide	Grey manganese Black manganese	$\ddddot{\mathrm{Mg}}$
	Wad	

	Silvery manganese	
Phosphate	Phosphate of manganese	
Carbonate	Compact red manganese	$\ddot{Mg}\,\ddot{C}^2 + 2\,H^2O$
Tungstate	Wolfram	$\ddot{Mg}\,\overset{\substack{\cdots\\\cdots}}{W} + 3\,\ddot{Fe}\,\overset{\substack{\cdots\\\cdots}}{W}$
Tantalate	Tantalite..................	$\ddot{Mg}\,\ddot{Ta} + \ddot{Fe}\,\ddot{Ta}$
Siliciates..........	Black mangankisel	$\ddot{Mg}\,1\tfrac{1}{2}\,\dddot{Si} + 3\,H^2O$
	Red mangankisel	$\ddot{Mg}\,1\tfrac{1}{2}\,\dddot{Si}^2$
	Pyrosmalite	

Seventeenth Family: Cerium.

Siliciate	Cerite	$\ddot{Ce}\,1\tfrac{1}{2}\,\dddot{Si}$

Division II.—*Metals which cannot be reduced by charcoal powder, and whose oxides form the earths and alkalies.*

First Family: Zirconium.

Siliciate	Zircon or hyacinth	Z S?

Second Family: Aluminium.

Sulphate	Native alumina	
Fluate.............	Wavellite?	
Fluo-siliciate.......	Pycnite	$A\,Fl + 3\,A\,S$
	Topaz	$A^2\,Fl + 3\,A\,S$
Siliciate	Sapphire	
	Ruby	
	Corundum	
	Emery	
	Collyrite...................	$A^3\,S + 5\,Aq$
	Nepheline	$A\,S$
	Disthene	$A\,S$
	Pitch-stone................	$A\,S^6 + Aq$?
	Steinheilite	
	Hisingrite	$A\,S + f\,S + 3\,F\,S$
	Pinite?	
	Staurolite	
	Almandine	
	Fahlun garnet	
	Rothoffite..................	$mg\,S + F^3\,S + 4\,A\,S$
	Manganese flint from Spessart	
Hydrates..........	Diaspore Oriental turquois Earthy wavellite	}
Clayholding*	Kaolin	
	Lithomarge	
	Mountain soap	
	Bole	
	Terra lemnia	
	Fuller's earth	
	Cimolite	
	Clay	
	Blue clay	
	Clay slate	
	Bituminous shale, &c.	

Third Family: Yttrium.

Fluate............	See Calcium	

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* Mixtures of aluminous silicates with foreign matters in powder.

Tantalate	Yttrotantalum	$Y^2 Ta$
	— containing tungsten	
	— containing uranium	
Siliciate	Gadolinite	$f^3 S + ce^3 S + 3 Y S$

Fourth Family: Glucinum.

Siliciates	Emerald	$G S^4 + 2 A S^2$
	— containing chromium	
	— containing tantalum	
	— containing tin	
	Euclase	$G S^2 + 2 A S + x$

Fifth Family: Magnesium.

Sulphate	Epsom salt	$\ddot{Mn}\,\dddot{S}^2 + 10 H^2 O$
Carbonate	Magnesite	$\ddot{Mn}\,\ddot{C}^2$
	Picrolite	
Borate	Boracite	$\ddot{Mn}\,\ddot{B}^4$
Siliciates	Steatite	$M S^3$
	Meerschaum	$M S^3 + 5 Aq$
	Precious serpentine	$M S^2 + Aq$?
	Serpentine	
	Chlorite	
	Soapstone	$M S^2 + A S^2 + 2 Aq$
	Nephrite	
	Indurated fahlunite	$M S^2 + 2 A S$
	Hyperstene	$M S^2 + 3 F S^2$?
	Bronzite	$F S^2 + 3 M S^2$?
	Olivine	$f S + 4 M S$?
	Pargasite	
	Lazulite	
Aluminates	Spinell	$M A^6$
	Pleonast	

Sixth Family: Calcium.

Sulphates	Auhydrous gypsum	$\ddot{Ca}\,\dddot{S}^2$
	— containing water	$\ddot{Ca}\,\dddot{S}^2 + 2 H^2 O$
Phosphate	Apatite	$\ddot{Ca}\,\ddot{P}^2$
Fluate	Flnor spar	$\ddot{Ca}\,\ddot{F}$
	Yttrocerite	
Carbonate	Calcareous spar	$\ddot{Ca}\,\ddot{C}^2$
	Bitter spar	$\ddot{Ca}\,\ddot{C}^2 + \ddot{Mn}\,\ddot{C}^2$
	Gurofian	$\ddot{Mn}\,\ddot{C}^2 + 3\,\ddot{Ca}\,\ddot{C}^2$
	Frankenhainer bitter spar	$\ddot{Ca}\,\ddot{C}^2 + 3\,\ddot{Mn}\,\ddot{C}^2$
	Arragonite	
Borosilicate	Datolite	$2\,C\,Bo^4 + 2\,C S^4 + H^2 O$
	Botryolite	$2\,C\,Bo^2 + 2\,C S^4 + H^2 O$
Arseniate	Pharmacolite	$\ddot{Ca}\,\overset{:\cdot:}{As} + 6 H^2 O$
Tnngstate	Tungsten	$\ddot{Ca}\,\overset{:::}{W}$
Silicio titaniate	Sphene	
Siliciate	Triple silicate from Edelfors	$C S^6$
	Table spar	$3 C S^2 + Aq$
	Lomonite	$C S^2 + A S^2 + 6 Aq$
	Mealy zeolite	$C S^3 + A S^3 + 4 Aq$
	Stilbite	$C S^3 + A S^2 + 6 Aq$
	Schorlous beryl scapolite	$C S + 3 A S$

	Borkhalts zeolite	$CS^2 + 3AS$
	Needle stone	$CS^3 + 5AS + 3Aq$
	Foliated prehnite	$FS + 3CS + 9AS + Aq$
	Radiated prehnite	$FS + 6CS + 15AS + 2Aq$
	Koupholite	$FS + 5CS + 9AS$
	Chrysoberyl	
	Malacolite **Grammatite**	$CS^2 + MS^2$
	Asbestos	$CS^3 + MS^2$
	Actynolite	$CS^2 + fS^2 + 3MS^2$
	Coccolite	$mgS^2 + 2fS^2 + 6MS^2 + 12CS^2$
	Byssolite	$CS^2 + MS^2 + mgS^2 + fS^2$
	Yenite	$CS + 4fS$
	Black garnet	$fS + 3FS + 3CS$
	Melanite	$fS + 3FS + 2AS + 6CS$
	Thuringian garnet	$CS + FS$
	Aplome	$CS + FS + 2AS$
	Grossularia	$fS + 3FS + 4AS + 12CS$
	Laboite	$MS + 2FS + 12AS + 15CS$
	Colophonite	$(MgS + 2FS +)MS + 3AS + 4CS$
	Dannemora garnet	$MgS + FS + CS + 2AS$
	Pyrope	$CS + 4MS + 6FS + 15AS$
	Allochroite	$MgS + fS + 3FS + AS + 6CS$
	Cinnamon stone, or Vesuvian	$FC + 4CS + 5AS$
	Idocrase	$FS + 5AS + 6CS$
	Axinite	$CS^2 + FS + 5AS$ $CS^2 + FS + 2AS$
	Brazilian tourmaline	$CS + 2fS + 18AS$
	Epidote	$CS + FS + 3AS$
	Scorza	$CS^2 + 3fS + 3AS$
	Zoisite	$FS + 5CS + 10AS$
	Antophyllite	$FS + 2CS + 4AS$
	Smaragdite	
	Augite	
	Schiller-stone	
	Hornblende	
	Allanite	

Seventh Family: Strontium.

Sulphate	**Schutzite**	$\ddot{Sr}\,\dddot{S}^2$
Carbonate	**Strontianite**	$\ddot{Sr}\,\ddot{C}^2$

Eighth Family: Barytium.

Sulphate	**Heavy spar** **Liver-stone**	$\ddot{Ba}\,\dddot{S}^2$
Carbonate	**Witherite**	$\ddot{Ba}\,\ddot{C}^2$
Siliciate	**Harmotome from Andreasberg**	$BS^4 + 4AS^2 + 7Aq$
	Oberstein	$BS^2 + 6AS^2 + 7Aq$

Ninth Family: Sodium.

Sulphate	**Glauber salt**	$\ddot{Na}\,\dddot{S}^2 + 20H^2O$
	Glauberite	$\ddot{Na}\,\dddot{S}^2 + \ddot{Ca}\,\dddot{S}^2$
Muriate	**Rock salt**	$\ddot{Na}\,\ddot{M}^2$
Borate	**Tinkal**	$\ddot{Na}\,\ddot{B}^6 + 36H^2O$
Fluate	**Kryolite**	$NFl + AFl$
Siliciate	**Sodalite**	$NS + 2AS$
	Lazurstein	$NS + 3AS$
	Mezotipe or natrolite	$NS^3 + 3AS + 2Aq$

	Electric schorl	$NS + 9AS$
	Scolezite	$NS^3 + 2CS^3 + 9AS + 9Aq$
	Cubizite	$CS^3 + 4NS^3 + 18AS^2 + 12Aq$
	Sarcolite	$CS^3 + NS^3 + 9AS^2 + 18Aq$
	Wernerite	$CS + NS + 24AS + 7Aq$
	Ekebergite	$NS^2 + 3CS^2 + 9AS$
	Scapolite	$NS^3 + 3MS^2 + 4CS^2 + 6AS^2$
	Light violet rubellite	$mgS + 2NS + 12AS$
	Dark violet rubellite	$mgS + NS + 6AS$
	Saussurite	$NS^2 + MS^2 + 2CS^2 + FS + 9AS$
	Labradore stone	$NS^3 + fS^3 + 3CS^3 + 9AS$
	Basalt	
	Clink-stone	

Tenth Family: Potassium.

Sulphate	Alum	$\ddot{K}\dddot{S}^2 + 2\dddot{Al}\dddot{S}^3 + 48H^2\bullet$
Nitrate	Saltpetre	$\ddot{K}\,\overset{\vdots\vdots}{N}^2$
Siliciates	Felspar	$KS^3 + 3AS^2$
	Leucite	$KS^2 + 3AS^2$
	Elœolite	$KS^2 + 4AS^2$
	Lepidolite	$KS^3 + 6AS^3 + Aq$
	White-stone	$KS^6 + 6AS^6$?
	Spodumene	$KS^3 + 12AS^3$
	Andalusite	$KS + 18AS$
	Tourmaline	$KS + 4fS + 15AS$
	Ichthyophthalm	$KS^3 + 8CS^3 + 16Aq$
	Chabasite	$KS^3 + NS^3 + CS^3 + 9AS + 18Aq$
	Mica	
	— Silver	$KS^3 + 2FS + 4AS$
	— Large foliated	$KS^3 + FS + 12AS$
	— Black	$KS^3 + FS + 3AS + 2MS$
	Talc	
	Agalmatolite	
	Green earth	
	Pumice	
	Porcelain jasper?	
	Obsidian	

CLASS II.

Contains bodies formed according to the principles of organized nature; that is, in which the compounds of the first order contain more than two elements.

Order I. Evidently putrefied organic Bodies.

Humus,
Turf,
Brown coal.

Order II. Resinous Bodies.

Amber,
Retinasphalt,
Mineral caoutchouc.

Order III. Liquids.

Naphtha,
Petroleum.

Order IV. Pitchy Bodies.

Maltha,
Asphalt.

Order V. Coals.

Branderz,
Stone coal.

Order VI. Salts.

Sulphate of ammonia,
Sal-ammoniac,
Mellite.

Such is the arrangement of minerals which Berzelius has given. The formulas which accompany most of the species will enable the reader to judge of the composition of each according to the view of the subject taken by Berzelius. These formulas have been explained by Professor Berzelius himself in the *Annals*, iii. 51, and in his Attempt to establish a pure scientific System of Mineralogy, p. 45 (English translation). A paper on this subject by Dr. Schubert has been published in Schweigger's Journal, xv. 200, in which he endeavours, from the best analyses hitherto made, to determine by the rules of Berzelius the chemical composition of a great variety of minerals. This paper is entitled, Einige Biträge zu den stochiometrischen Berechnungen des Mischungsverhältnisses der Fossilien. Its length puts it out of my power to give an account of it here; but it deserves the attention of those persons who are engaged in studying this interesting but intricate subject. A comparison between the conclusions of Berzelius and Schubert will show how far they agree, and may serve in some measure to determine the degree of accuracy which has been already attained.

4. *Iolite.* — Dr. Leopold Gmelin has analyzed the iolite, or dichroite, a mineral brought from the Cap de Gate in Spain by a French mineral dealer, and first established as a peculiar species by Werner. He found its constituents as follows:—

Silica	42·6
Alumina	34·4
Magnesia	5·8
Lime	1·7
Protoxide of iron	15·0
Oxide of manganese	1·7
	101·2

The mineral called *saphir d'eau*, which comes from India in grains about the size of an almond, and usually pierced, was likewise analyzed by him. He found its constituents as follows:—

Silica	43·6
Alumina	37·6
Magnesia	9·7
Lime	3·1
Potash?	1·0
Protoxide of iron	4·5
Oxide of manganese	Trace
	99·5

From this analysis it is obvious that the saphir d'eau is not a variety of quartz, as has been hitherto pretty generally supposed. It appears to be nearly related to iolite. (See Schweigger's Journ, xiv. 316.

5. *Magnesite.*—This is the name given by mineralogists to native carbonate of magnesia, which was first discovered by Dr. Mitchell. Professor Haussmann has lately discovered a new subspecies of this

mineral from Baumgarten, in Silesia. Its characters as he has described them are as follows:—

The fresh fracture is snow-white; but by exposure to the atmosphere, it becomes yellowish-white.

Fracture fine-grained uneven; sometimes passing into the splintery and the even.

Fragments irregular and sharp edged.

Lustre dull. None is produced by rubbing the mineral with the nail.

Slightly translucent on the edges.

Very difficultly frangible.

Scratches fluor spar and glass, and frequently gives slight sparks with steel.

Does not adhere to the tongue. Sp. gr. 2·95.

According to the analysis of Stromeyer, this mineral is composed of

Magnesia	47·6334
Carbonic acid	50·7643
Oxide of manganese	0·2117
Water	1·3906
	100·0000

Hence we see that this mineral is composed of an atom of magnesia united to an atom of carbonic acid. The water does not appear to be chemically combined with the carbonate of magnesia. (See Schweigger's Journal, xiv. 1.)

6. *Anhydrite.*—Stromeyer has likewise published the analysis of a variety of fibrous anhydrous sulphate of lime from Himmelsberge about half a German mile south-west of Ilefeld, where it occurs in a bed of the older floetz gypsum. Its constituents were,

Lime	40·673
Sulphuric acid	55·801
Carbonic acid	0·087
Oxide of iron	0·254
Silica	0·231
Bitumen	0·040
Water	2·914
Common salt	Trace
	100·000

Or it contains,

Anhydrous sulphate of lime	85·877
Hydrous sulphate of lime	13·400
Carbonate of lime	0·198
Other bodies	0·525
	100·000

(See Schweigger's Journal, xiv. 375.)

6. *Gehlenite.*—This is a name given by the Germans to a mineral described and analyzed by Professor Fuchs, in Schweigger's Journ.

xv. 377. It occurs usually crystallized in four-sided rectangular prisms, whose bases are squares. They are always so low that they belong to the kind denominated by the Germans *table*; that is, the sides of the prism are smaller than its bases. They are commonly small, never exceeding 4½ lines in length, and 6¼ in breadth. They are entangled in each other, and the intervals between them are usually filled up with calcareous spar. They have a triple cleavage, two of which can be readily distinguished; but the third with difficulty.

The specific gravity is 2·98. It is moderately easily frangible, and semihard in a high degree, scratching glass, but not striking fire with steel.

The fracture is sometimes uneven, sometimes fine splintery. Lustre weakly glimmering, or almost dull. The kind of lustre is intermediate between vitreous and resinous.

It has no very decided colour; but its principal colour is intermediate between olive and leek green; and passes on the one side through dark bluish-grey to bluish-black, and on the other to dark oil-green or liver-brown.

It is usually only translucent on the edges, sometimes quite opake, and only the very small crystals are entirely translucent. The crystals have somewhat of a resinous feel. The powder feels meagre.

Before the blow-pipe it melts with difficulty into a yellowish-green bead, which has some degree of transparency. When the flame is long continued, it becomes black.

According to the analysis of Fuchs its constituents are,

Silica	29·64
Alumina	24·80
Lime	35·30
Oxide of iron	6·56
Water	3·30
	99·60

7. *Carbo-silicate of Copper.*—In the *Annals*, vii. 321, I have described a new species of copper ore, which Mr. Mawe received from Mexico, and gave likewise an analysis of it. An anonymous correspondent has rendered it probable that it is a combination of carbonate of copper and silicate of copper. He has shown likewise that dioptase is a combination of hydrate of copper and silicate of copper. (*Annals*, viii. 151.) The symbols for these two species of copper ore will be as follows:—

Carbo-silicate.............. $Cu\ C + Cu\ S^3$
Hydro-silicate .... $Cu\ H^2 + Cu\ S^3$

8. *Aachen Mass of Iron.*—In the *Annals*, vi. 53, I inserted a short historical account of the celebrated Aachen mass of iron. Since that time a particular description of this mass has been published by Berg-Commissär Noeggerath, together with an elaborate chemical analysis of it by Dr. J. P. J. Monheim. (Schweigger's Journal, xvi. 196, July, 1816.)

The specific gravity of this mass is 6·723, and its absolute weight is above 7400 lb. It is coated with a crust of oxide of variable thickness. The fresh fracture is tin-white, and has completely the metallic lustre. The fracture is fine granular, uneven, often scaly, and sometimes approaching small foliated. It is attracted by the magnet, and is itself possessed of magnetic properties. From the analysis of Monheim, its constituents appear to be,

Iron	500·5	83·42
Arsenic	90·0	15·0
Silicon	4·5	0·75
Carbon	3·0	0·5
Sulphur	2·0	0·33
	600·0	100·00

9. I have now to state the numerous analyses that have been inserted by Berzelius and Hisinger in the fourth volume of the Afhandlingar i Fisik Kemi och Mineralogi, published at Stockholm in 1815.

1. *Examination of some Minerals found in the Vicinity of Fahlun,* by John Gottlieb Gahn and Jacob Berzelius.

1. Found at Finbo.

(1.) *Yttrocerite.*—This appears to be a compound of fluate of lime, fluate of cerium, and fluate of yttria. Its colour is various, violet, greyish-red, white, grey, often all mixed in the same specimen. In amorphous masses, varying in size from a thin crust to half a pound in weight, disseminated through quartz. Fracture foliated. Lustre glistening. Opake. Scratched by the knife and by quartz. Scratches fluor spar. Sp. gr. 3·447.

Before the blow-pipe it loses its colour, and becomes white; but does not fuse of itself; but when mixed with gypsum readily melts into a bead. When in fine powder, it dissolves completely in boiling muriatic acid, and the solution has a yellow colour. Its constituents are,

	Lime	47·63	to 50·00
	Yttria	9·11	to 8·10
	Oxide of cerium	18·22	to 16·45
	Fluoric acid	25·05	to 25·45
		100·01	100·00
Or	Fluate of lime	65·162	to 68·18
	Fluate of yttria	11·612	to 10·60
	Fluate of cerium	23·226	to 20·22
		100·000	99·00

(2.) *Tin-stone.*—Crystals of tin-stone are found imbedded in quartz. They are black, with a shade of red or reddish-grey. Sometimes crystallized in octahedrons, but most frequently in small grains. Fracture uneven. Lustre metallic. Opake. Hard. Scratches glass. Sp. gr. 6·55. Not altered before the blow-pipe per se. Its constituents are,

Oxide of tin	93·6
Oxide of tantalum	2·4
Oxide of iron	1·4
Oxide of manganese	0·8
	98·2

(3.) *Tantalite.*—Darker grains were occasionally found imbedded in the quartz, which, when reduced before the blow-pipe, yielded a smaller proportion of tin. They were at first taken for tin-stone; but after the discovery of tantalite at Broddbo, they were more closely examined, and found to consist of a mixture of tin-stone and tantalite. One variety being analyzed, gave

Oxide of tantalum	66·99
Oxide of tin	16·75
Oxide of iron	7·67
Oxide of manganese	7·98
Lime	2·40
	101·79

Berzelius considers it as a compound of

Tantalite	67·5
Tantalate of lime	15·4
Tin-stone	17·1
	100·0

Another variety gave,

Oxide of tantalum	12·22
Oxide of tin	83·65
Oxide of iron	2·18
Oxide of manganese	1·22
Lime	1·40
	100·67

According to Berzelius, it consists of

Tin-stone	85·3
Tantalite	14·7
	100·0

(4.) *Emerald*, or rather *Pseudo-emerald.*—This mineral is found in regular six-sided prisms, from one to three inches in diameter. The colour varies from dark green to yellow green. Fracture uneven, and either dull, or having a weak resinous lustre. Translucent on the edges. Easily scratched with a knife. Sp. gr. 2·701. From the quantity of glucina which it contains, Berzelius considers it as consisting of

Emerald	59
Talc	41
	100

(5.) *Slaty Talc.*—Colour varying from grey green to brown green. Found in amorphous masses. Fragments rhomboidal. Fracture foliated. Lustre waxy. Translucent on the edges. Easily scratched by the knife. Sp. gr. 2·718. Its constituents are,

Silica	51·40
Alumina	33·16
Oxide of iron	4·00
Lime with some magnesia	3·00
Loss	8·44
	100·00

2. Found at Broddbo.

(1.) *Emerald.*—Colour varying from bluish-green to yellowish-green. Crystallized in regular six-sided prisms. Fracture uneven and splintery. Lustre resinous. Opake, or translucent only when in thin fragments. Hard. Scratches quartz. Sp. gr. from 2·673 to 2·683. Its constituents are,

Silica	68·35
Alumina	17·60
Glucina	13·13
Oxide of iron	0·72
Oxide of tantalum	0·27
	100·07

If we consider the metallic oxides as foreign bodies, then the emerald will be composed of

Silica	68·64
Alumina	17·96
Glucina	13·40
	100·00

Its symbol will be $G S^4 + 2 A S^2$.

(2.) *Tantalite.*—Colour black, and equal. Surface often polished. In amorphous masses, without any tendency to crystallization. Fracture uneven. Lustre metallic. Fragments indeterminate. Opake. Scratches glass. Gives no sparks with steel, and is scratched by quartz. Sp. gr. in large masses 6·291; in smaller portions 6·208. Has no perceptible action on the magnet. Insoluble in acids. Not altered before the blow-pipe per se. But with phosphate of soda or borax it fuses into a yellowish glass. With soda it gives grains of tin, especially if a little borax be added. According to the experiments of Berzelius, different specimens were composed as follows:—

Oxide of tantalum	66·66	68·22	66·345
Tungstic acid	5·78	6·19	6·120
Oxide of tin	8·02	8·26	8·400
Oxide of iron	10·64	9·58	11·070
Oxide of manganese	10·20	7·15	6·600
		1·19	1·500 lime
	101·30	100·59	100·035

Berzelius considers it as composed as follows :—

Tantalite	Oxide of tantalum ..	67·586	82·552
	Oxide of manganese..	5·902	
	Oxide of iron	7·560	
	Lime	1·504	
Wolfram			8·690
Tin-stone			8·758
			100·000

Tantalite, therefore, is composed of

Oxide of tantalum	81·872
Oxide of iron	9·178
Oxide of manganese	7·124
Lime	1·826
	100·000

Its symbol, therefore, is $\ddot{\mathrm{Ca}}\ \ddot{\mathrm{Ta}} + 3\ \ddot{\mathrm{Mn}}\ \ddot{\mathrm{Ta}} + 4\ \ddot{\mathrm{Fe}}\ \ddot{\mathrm{Ta}}$, or $(\ddot{\mathrm{Ca}}\ \ddot{\mathrm{Ta}} + \ddot{\mathrm{Fe}}\ \ddot{\mathrm{Ta}}) + 3\ (\ddot{\mathrm{Mn}}\ \ddot{\mathrm{Ta}} + \ddot{\mathrm{Fe}}\ \ddot{\mathrm{Ta}})$.

II. *Examination of the Composition of Gadolinite*, by J. Berzelius. This is a curious and elaborate analysis. He shows that yttria had never been completely freed from cerium; and that when obtained in a state of purity, it is white, and forms colourless salts with acids. Sulphate of yttria is composed of 100 acid + 100 yttria. Hence yttria contains 20 per cent. of oxygen. So that if we consider it as a protoxide, the weight of an atom of yttrium will be 4; but if yttria be a deutoxide, the weight of an atom of yttrium will be 8.

Gadolinite from Finbo was composed of

Silica	25·80
Yttria	45·00
Protoxide of cerium	16·69
Protoxide of iron	10·26
Water	0·60
	98·35

Gadolinite from Broddbo, of

Silica	24·16
Yttria	45·93
Protoxide of cerium	16·90
Protoxide of iron	11·34
Water	0·60
	98·93

Hence its symbol, according to Berzelius, is $f^3\ S + ce^3\ S + 8\ Y\ S$.

III. *Analysis of the Fluosilicates hitherto discovered, or of the Minerals arranged under the Species Topaz.* By J. Berzelius.—The minerals analyzed by Berzelius were the Brazilian topaz, the Saxon topaz, and the pyrophysalite. The results were as follows :—

	Alumina.	Silica.	Fluoric Acid.	Total.
Brazilian topaz	58·38	34·01	7·79	100·18
Saxon topaz	57·45	34·24	7·75	99·44
Pyrophysalite	57·74	34·36	7·77	99·87

From these analyses it is obvious that these minerals belong to one and the same species. If we suppose the topaz composed thus, $A^2\,Fl + 3\,A\,S$, then its constituents will be

Alumina	58·55
Silica	34·27
Fluoric acid	7·18
	100·00

Schorlous beryl, stangenstein, or pycnite (for it is known by all these names) was found composed of

Alumina	51·00
Silica	38·43
Fluoric acid	8·84
	98·27

If we consider it as composed of $A\,Fl + 3\,A\,S$, then its constituents will be

Alumina	53·07
Silica	38·80
Fluoric acid	8·13
	100·00

IV. *Experiments to determine the Composition of the Minerals at present known to contain Tantalum.* By J. Berzelius.

(1.) *Tantalite from Finland.*—Berzelius analyzed some specimens of the original tantalite from Finland, examined long ago by Ekeberg, which he received from Dr. Macmichael. A piece of the mineral labelled by Ekeberg as of the sp. gr. 7·236, yielded the following constituents:—

Oxide of tantalum	83·2
Protoxide of iron	7·2
Protoxide of manganese	7·4
Oxide of tin	0·6
	98·4

Hence its symbol is mg Ta + f Ta.

(2.) *Yttrotantalite from Ytterby.*—This mineral was first examined by Ekeberg, who ascertained the nature of its constituents; but it has neither been accurately analyzed nor described. There are three varieties of it, which Berzelius describes and examines separately.

A. *Black Yttrotantalite.*—It occurs in a rock composed of red felspar and mica, together with gadolinite, in masses never larger than a hazel nut, and which sometimes exhibit traces of crystallization. Colour black. Fracture foliated. Lustre strongly glistening, metallic. Fragments indeterminate. Easily frangible. Gives a grey powder. Opake. Hard. Scratches glass. Sp. gr. 5·395.

Before the blow-pipe it decrepitates feebly, becomes dark brown, but does not fuse. It dissolves slowly in phosphate of soda, forming a safron-coloured glass. In borax it dissolves with more facility. It is insoluble in acids. Its constituents were,

Oxide of tantalum	57·00
Tungstic acid	8·25
Yttria	20·25
Lime	6·25
Peroxide of iron	3·50
Peroxide of uranium	0·50
	95·75

B. *Yellow Yttrotantalite.*—Colour yellowish brown, frequently with greenish streaks and lines. It usually forms a crust upon felspar; but sometimes occurs in grains, never exceeding the size of a peppercorn. Fracture foliated. Lustre of the cross fracture vitreous, of the principal fracture resinous. Opake. Gives a white powder. It scarcely scratches glass. Sp. gr. according to Ekeberg 5·882. Before the blow-pipe it decrepitates feebly; but does not melt; and the colour becomes light straw-yellow. Analyzed in different ways, it gave the following constituents:—

Oxide of tantalum	60·124	59·50
Yttria	29·780	24·90
Lime	0·500	3·29
Oxide of uranium	6·622	3·23
Oxide of iron	1·155	2·72
Tungstic acid and tin	1·044	1·25
	99·225	94·89

C. *Dark Yttrotantalite.*—Colour black, with a slight shade of brown. Found in the same state as the preceding variety. Fracture in one direction glassy; in another fine granular. Lustre between vitreous and resinous. Translucent when very thin. Gives a white powder. Hardness the same as that of the preceding variety. Sp. gr. not determined. Does not melt before the blow-pipe, but decrepitates feebly. Its constituents were found to be,

Oxide of tantalum	51·815
Yttria	38·515
Lime	3·260
Oxide of uranium	1·111
Tungstic acid holding tin	2·592
Oxide of iron	0·555
	97·848

In Berzelius's opinion, black yttrotantalite is Y^2 Ta mixed with C^2 Ta and with F W^3.* The yellow variety is Y^2 Ta mixed with a small portion of C^2 Ta and U^2 Ta with a trace of wolfram. The dark variety is Y^3 Ta mixed with a small portion of C^3 Ta and U^3 Ta, together with a trace of wolfram.

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* W is Berzelius's symbol for tungsten.

V. *Experiments to determine the Composition of the Minerals containing Tungsten.* By J. Berzelius.

(1.) *Wolfram.*—The constituents of this mineral as determined by Berzelius, are

Tungstic acid	78·775
Protoxide of iron	18·320
Protoxide of manganese	6·220
Silica	1·250
	104·565

He considers it as $\overset{\cdot\cdot}{Mg}\,\overset{\vdots\vdots\vdots}{W} + 3\,\overset{\cdot\cdot}{Fe}\,\overset{\vdots\vdots\vdots}{W}$.

(2.) *Tungstate of Lime.*—This scarce mineral he found composed of

Tungstic acid	80·417
Lime	19·400
	99·817

So that it is $\overset{\cdot\cdot}{Ca} + \overset{\vdots\vdots\vdots}{W}$.

VI. *Chemical Analysis of different Minerals.* By W. Hisinger.

(1.) *Pyrodmalite from the Mines of Nordmark.*—Its constituents were,

Silica	35·850
Protoxide of iron	21·810
Protoxide of manganese	21·140
Submuriate of iron	14·095
Lime	1·210
Water and loss	5·895
	100·000

(2.) *Cerine.*—Its constituents were,

Silica	30·17
Alumina	11·31
Lime	9·12
Oxide of cerium	28·19
Oxide of iron	20·72
Oxide of copper	0·87
Volatile matter	0·40
	100·78

Hisinger considers it as $C\,S^2 + 2\,A\,S^2$ mixed with $ce\,S^2 + fe\,S^2$. The common cerite of mineralogists is a mixture of hornblende and true cerite.

(3.) *A yellow Mineral from Longbanshyttan.*—Probably augite mixed with oxide of manganese. Its constituents were,

Silica	52·80
Lime	13·76
Magnesia	12·40
Oxide of manganese	8·30
Oxide of iron	2·00
Volatile matter	8·74
	98·00

Its symbol is $mg\ S^3 + 2\ C\ S^3 + 3\ M\ S^3 + 4\ Aq$.

(4.) *Precious Serpentine from Skytt Mine, near Fahlun.*—Its constituents were,

Silica	43·07
Magnesia	40·37
Oxide of iron	1·17
Lime	0·50
Alumina	0·25
Volatile matter	12·45
Oxide of manganese	Trace
	97·81

Dr. John had previously analyzed this mineral, and found its constituents,

Silica	42·5
Magnesia	38·6
Oxide of iron	1·5
Water	15·2
	97·8

Hisinger conceives that the precious serpentine may be $2\ M\ S + Aq$ mixed with $S^2\ Aq$.

(5.) *A yellowish brown Stone from Fahlun, called Hard Fahlunite.*—Its constituents were,

Silica	45·90
Alumina	31·10
Magnesia	13·50
Oxide of iron	3·00
Oxide of manganese	0·50
Lime, zinc	0·20
Volatile matter	3·00
	97·20

Hisinger considers it as $M\ S^2 + 2\ A\ S$.

(6.) *Iron Flint.*—Its constituents were,

Silica	90·00
Peroxide of iron	3·99
Lime and manganese	5·15
Alumina	Trace
	99·14

(7.) *Clay containing Chromium, from Mortenberg.*—Its constituents were,

Alumina	36
Silica	39
Oxide of chromium	10
Oxide of iron	3
Water	8
	96

(8.) *A greenish prismatic crystallized Mineral from the Mines of Nardmark.*—It is nearly related to axinite. Its constituents are

Silica	41·50
Lime	25·84
Alumina	13·56
Oxide of manganese	10·00
Oxide of iron	7·36
Volatile matter	0·30
	98·56

(9.) *Stilbite.*—Its constituents were,

Silica	58·0
Alumina	16·1
Lime	9·2
Iron and manganese	Trace
Volatile matter	16·4
	99·7

Hence stilbite is $C S^3 + 3 A S^3 + 6 Aq$.

(10.) *Liver-brown Copper Pyrites.*—Its constituents were,

Copper	63·334
Iron	11·804
Sulphur	24·696
Quartz	0·166
	100·000

Hence it is $Fe S^2 + 4 Cu S$.

(11.) *Carbonate of Manganese and Lime.*—Its constituents were,

Carbonate of lime	74·75
Carbonate of manganese	21·00
Carbonate of magnesia	4·27
	100·02

(12.) *Pearlspar.*—Its constituents were,

Lime	27·97
Magnesia	21·14
Oxide of iron	3·40
Oxide of manganese	1·50
Carbonic acid	44·60
	98·61

(13.) *Lime-stone from Pehrshyttan, near Nora.*—This is a primitive lime-stone, white, thick, and mixed here and there with streaks of greenish tremolite. Its constituents were,

Lime	34·80
Magnesia	15·56
Carbonic acid and water	45·28
Oxide of iron	1·76
Oxide of manganese	0·60
	98·00

(14.) *Grammatite from Fahlun.*—Its constituents were,

Silica	59·244
Magnesia	22·133
Lime	15·200
Oxide of iron	1·311
Oxide of manganese	1·000
Alumina	0·888
Water	0·020
	99·796

Hence its mineralogical formula is $C S^3 + 2 M S^2$.

VII. *Analysis of red Manganese Ore (Mangankisel), from Longbanshyttan.* By J. Berzelius. Its constituents were,

Silica	48·00
Oxide of manganese	54·42
Lime	3·12
Magnesia	0·22
Oxide of iron	Trace
	105·76

Berzelius considers it as composed of

Bisilicate of protoxide of manganese	93·288
Bisilicate of lime	6·712
	100·000

VIII. *Analysis of Fahlun Garnet.* By W. Hisinger. Its constituents were,

Silica	39·66
Alumina	19·66
Protoxide of iron	39·68
Oxide of manganese	1·80
	100·80

He considers it as $A S + f S$.

IX. *Analysis of a new Variety of Gadolinite from Korarfvet, in the Neighbourhood of Fahlun.* By J. Berzelius. Its constituents were,

Silica	29·18
Yttria	47·30
Oxide of iron	8·00
Lime	3·15
Glucina	2·00
Oxide of cerium	3·40
Oxide of manganese	1·30
Water	5·20
	99·53

Berzelius considers it as composed of

True gadolinite	83·67
Bisilicate of lime	7·27
Silicate of glucina	2·90
Silicate of cerium	4·33
Silicate of manganese	1·83
	100·00

II. GEOGNOSY.

This historical sketch has already extended to such a length, that I find it impossible to enter into the details which this fashionable and prolific branch of mineralogy would require. I shall, therefore, leave out for the present every thing that is contained in the third volume of the Transactions of the Geological Society, as I intend to give an analysis of that volume in a subsequent number of the *Annals*. A very small number of topics, therefore, will be touched upon here.

1. *Mineralogical Surveys.*—It is scarcely necessary to observe, that all real progress in geognosy depends upon an accurate knowledge of the structure of the earth. While idle speculations about the formation of the earth lead to nothing better than wrangling and confusion, every new fact respecting the relative position of rocks, every accurate description of a district, adds somewhat to our former knowledge, and contributes towards the completion of the science. Geognosy will be complete only when we are accurately acquainted with the structure of the whole surface of the globe, and when we understand completely the laws which regulate the changes which it is slowly undergoing. Nothing, therefore, is of more importance than accurate mineralogical surveys of every county of Great Britain, provided these surveys be conducted by men adequate to the undertaking. The plan sketched by Professor Jameson, and published in the *Annals*, vii. 102, will serve as a very good model of what these surveys ought to be, while his mineralogical survey of Dumfrieshire will show how much can be accomplished by one man within a moderate time.

2. *New Arrangement of Rocks.*—In the *Annals*, vii. 478, I have given the *formations* in the Riesengebirge as determined by the celebrated German geologist Raumer. They are as follows :—

1. Central granite.
2. Gneiss and granite.
3. Green-slate.
4. Gneiss.
5. Mica-slate.
6. Clay-slate.

3. *Country round Birmingham.*—In the *Annals*, viii. 161, I have given a sketch of the structure of the country round Birmingham. The lowest formation known is a floetz lime-stone, which rises through the surface, and forms hills at Dudley, and is quarried likewise near Walsal. Over this lies the coal formation which begins at Stourbridge, and extends about 16 miles north-east, with a breadth of about four miles. Over the coal formation lies a range of low basalt hills, extending from Dudley towards Hales Owen. The county of Warwick, and that part of Worcester which lies near it, consist of a red sand or sand-stone covering the coal, and full of pebbles which appear water-worn. The Birmingham coal, as far as I know, constitutes the only well-known example in Great Britain of the coal formation lying immediately over floetz lime-stone. It would be an object of some interest to determine whether the same position exists in any other coal-field.

4. *Cumberland.*—Though this county has been visited by many mineralogists, we are not yet in possession of a correct delineation of its structure. On that account I think it worth while to mention a notice respecting some of the rocks which occur in this county, published in the Philosophical Magazine, xlvii. 41. It is not of a nature to be epitomized. I must, therefore, satisfy myself with referring to it. But it will be of considerable use to those mineralogists who may hereafter undertake a description of this intricate country. I believe a good deal of the intricacy arises from the porphyry which caps many of the mountains, and which changes its aspect so much in different places, that considerable attention is requisite in order to recognize it.

5. *Level of the Caspian and Black Sea.*—From the observations of Engelhardt and Parrot, made with great care, it appears that the surface of the Caspian is lower than that of the Black Sea by about 99 metres, or 324·7 English feet.

6. *Matrix of Cinnamon Stone.*—I have given a short description of the rock, in which the cinnamon stone occurs in the island of Ceylon, from a specimen which Mr. Mawe was so obliging as to send me. It is an aggregate of tabular spar (schaalstein), quartz, and cinnamon-stone. The rock is very beautiful. It is not unlikely that it may in situ be a quartz rock, in which the tabular spar and cinnamon stone are imbedded; but this can only be verified by an examination on the spot. It is to be expected that Dr. John Davy, who I believe has gone to Ceylon, will shortly furnish valuable information respecting the structure of this curious island. (See *Annals*, vii. 242.)

7. *Soda Lake in South America.*—It appears from a paper pub-

lished in the Journal of the Royal Institution, i. 188, that in Maracaybo, one of the provinces of Venezuela, 48 miles east of Merida, in about N. lat. 8°, and W. long. 70° and some minutes, there exists a small lake, from which a very considerable quantity of carbonate of soda is obtained once in two years. The salt crystallizes at the bottom of the lake, and is obtained by diving. This lake, it would appear, is usually nearly saturated with the salt. It contains no animals whatever.

VIII. METEOROLOGY.

1. *New portable Barometer.*—In the Ann. de Chim. et Phys. i. 113, Gay-Lussac has proposed a new portable barometer, which seems entitled to considerable attention, as it may be made very light, is of easy execution, and of course may be procured at a comparatively easy rate. It consists of a glass tube of the usual size, which continues cylindrical to A. (See Plate LX., Fig. 2.) Here a capillary glass tube is joined to it, whose internal diameter does not exceed one or two millimetres (0·039 to 0·078 inch). This tube is bent upwards near its extremity, and united to a short tube of the same diameter as the upper part of the long tube. The short tube is shut at its upper extremity; but has a capillary hole, B, made in it, which allows a free entrance to the air without permitting the mercury to spill. Such is the outline of the contrivance. It may be fitted up at pleasure, according to the fancy of the proprietor.

The barometer of Gay-Lussac is a syphon one. Dr. Bischof, of Erlangen, has published (Schweigger's Journal, xv. 387) a cheap method of constructing the common barometer, consisting of a straight glass tube plunged into a vessel containing mercury. He has also given a table of the correction of the length of the column of mercury for every degree of temperature. This is a correction that ought to be attended to in common meteorological observations. I believe, if this correction were always made, that barometers in different places would be found to correspond with each other much more nearly than they appear to do at present. It is unnecessary to insert Bischof's table here, as any person can easily construct a similar one for himself.

2. A very remarkable phenomenon took place at the town of Gerace in Calabria, on the 13th of March, 1813. The circumstance is related by Professor Sementini of Naples, and was published in the Bibliotheque Britannique; but I take it from the German translation published in Schweigger's Journal, xiv. 130. The wind was westerly, and heavy clouds over the sea were approaching the land. About two hours after noon the wind fell, and the sky became quite dark. The clouds assumed a red and threatening appearance, thunder followed, and rain fell, which had a red colour from a mixture of red dust. The inhabitants were alarmed and flocked to the churches, conceiving that the end of the world was come. The red dust was very fine. It became black

when exposed to a red heat, and effervesced when treated with acids. Its constituents were silica, carbonate of lime, alumina, iron, and chromium. What renders this rain the more remarkable is, that the constituents of this red dust are the same nearly with one of the varieties of the meteoric stones. Hence it probably had a similar origin. This fact destroys the plausibility of the hypothesis that derives the meteoric stones from the moon. It is equally hostile to the supposition, that they were bodies floating about in free space, unconnected with the solar system. The formation of the powder must have taken place in the atmosphere.

3. A new variety of meteoric stone fell on the third of October, 1815, at Langres, in France. The phenomena are described by M. Pistollet, a physician in that city (Ann. de Chim. et Phys. i. 45.) From the analysis of Vauquelin its constituents appear to be,

Silica	33·9
Oxide of iron	31·0
Magnesia	32·0
Chromium	2·0
	98·9

4. On the 11th of January, 1815, a very remarkable thunder storm took place in the Low countries and Westphalia It was uncommon on account of its great extent and the great number of places struck by lightning nearly at the same time. It extended in length from Antwerp to Minden, or about 200 miles, and in breadth from Bonn to Nimeguen, which cannot be less than 75 miles. It struck no fewer than 24 places within this great space, and set fire to several, although provided with good conductors. See Benzenberg's account of it in Gilbert's Annalen, l. 341.

5. The great quantity of moisture that sometimes exists in the atmosphere at very low temperatures is not easily reconciled to the common theory of vapour, unless we suppose that it has assumed the state of a liquid; but that in consequence of being charged with negative or positive electricity, the particles cannot unite together, and are each so very minute as not to be able to overcome the resistance of the atmosphere. A striking example took place in Westphalia on Nov. 4, 5, and 6, 1814. The thermometer was at 25¼°, and the weather was foggy. A weak north-east wind drove the fog against the trees, where it froze, and loaded them so much that tall firs of three feet in diameter were completely overturned and rooted up by the weight. (See Gilbert's Annalen, lii. 233.) A similar fog existed at London between Dec. 27 and Jan. 2 of the same winter. Fortunately there was no perceptible wind, otherwise the injury sustained by the trees might have been as great here as it was in Westphalia.

6. The following table exhibits the mean temperature of every month during 1815, in the different places of Great Britain in which meteorolgical tables have been kept and published.

	Plymouth.	Tottenham.	London.	Perth.	Kinfauns Castle.
January	33·4	32·76	34·0	31·9	32·19
February	45·6	44·49	43·6	39·9	40·71
March	47·0	47·28	47·6	40·2	41·06
April	49·0	48·56	49·4	44·6	44·79
May	56·3	58·71	58·2	52·0	52·44
June	60·3	59·88	61·6	56·4	56·36
July	61·9	61·58	62·9	58·1	58·19
August	63·4	62·04*	63·5	57·8	57·90
September	59·2	55·53	64·7	53·3	54·04
October	52.6	49·70	53·2	47·3	48·40
November	41·3	38·66	41·2	36·3	38·00
December	38·2	35·09	38·7	32·0	33·10
Annual Mean	50·68	49·52	51·6	45·8	46·465

On comparing this table with that in the *Annals*, vii. 67, it will be seen that 1815 was considerably warmer than 1814. The temperature of London, given in the third column, from the registers of the Royal Society, is certainly too high, as the Society has not a Six's thermometer, and no observations are made during the night. I believe the mean temperature of London to be rather under 50°; but certainly very little under it. The mineral spring at Tunbridge Wells is constantly of the temperature 50°, which undoubtedly represents the mean temperature of the year in that place. Now London, being further north, is probably a little colder. From 1814 and 1815 it would appear that the mean temperature of Plymouth is about 1·5° higher than that of London. This difference falls chiefly on the autumn and winter months, and is no doubt partly owing to the difference of latitude, and partly to the neighbourhood of the sea. The mean temperature at Perth is 45·4°.

The quantity of rain which fell in 1815 in different parts of Great Britain is as follows:—

Plymouth	34·10 inches
Tottenham	20·71
London	12·968†
Perth	20·754
Kinfauns Castle, 129 feet above the sea	18·00
Ditto garden, 20 feet	24·20
Ditto on a conical detached hill, 600 feet	45·70

The summer and autumn of 1815 were remarkably dry, and the weather delightful, yet the crop was excellent.

IX. ARTS.

I have so little room to spare, that I can merely name some of the most striking improvements in the arts which have been proposed or adopted during the preceding year. The subject is a copious one, had I taken it up at an earlier part of this historical sketch.

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* The first ten days of this month are wanting in Mr. Howard's table, *Annals*, vi. 318.

† The rain-gage is 114 feet above the low water level at Somerset House.

1. *Lighthouse with Parabolic Reflectors.*—Lighthouses, scattered in such abundance on the coast of Great Britain, now commonly consist of an Argand's lamp attached to a parabolic reflector. But probably it is not generally known that the smaller the wick is, the greater is the light which the reflector throws out. But this follows from a set of observations made by MM. Charles, de Rossel, and Arago, Commissioners of the French Academy, on a set of lamps with reflectors, made by M. Lenoir. Three lamps were used with diameters of 16, 12, and 6 lines, respectively. The last gave the best light, and did not consume one half of the oil. (Ann. de Chim. xcvi. 59.)

2. The Marquis de Chabanne's method of ventilating houses, of which an account will be found in the *Annals*, vii. 113, is very ingenious, and seems particularly calculated to secure the comfort of invalids.

3. So much has already been said concerning the ingenious method of preventing explosions in coal-mines contrived by Sir H. Davy, that it seems unnecessary to make any further observations on the subject here.

4. The two papers by Professor Schubler on the physical and chemical analysis of soils, published in the *Annals*, vii. 207, and viii. 115, claim the attention of the farmer, and seem well calculated to throw light upon the nature of soils. A subject of great importance, but still rather obscure.

5. Mr. Gregor's remark about Mr. Tennant's discovery, that wootz owes its peculiar qualities to the presence of a small quantity of arsenic, may probably contribute materially to the improvement of steel in this country.

X. PHYSIOLOGY.

Respecting this branch of science likewise, I am unfortunately precluded from entering into details. I shall barely mention a few of the most remarkable particulars that have attracted the attention of physiologists during 1815.

1. One of the most curious treatises connected with vegetable physiology which has appeared for a long time is, the introduction to Humboldt's Plants of South America. It respects the distribution of plants in the different continents, and is scarcely susceptible of being epitomized. But I have given a very full account of this introduction in the *Annals*, vii. 373, to which I beg leave to refer the reader.

2. In some of the newer formations lying over the chalk both in France and England, it is not uncommon to find both fresh and salt water shells in the same bed. This circumstance induced M. Beudant to make a series of experiments, in order to determine whether fresh water molusca could live in salt water, and *vice versa*. He found that when fresh water molusca were suddenly introduced into water containing four per cent. of common salt, which is the case with sea water, they die in a very short time.

But if the fresh water is very gradually impregnated with salt, they can be made to live in it when of the strength of sea water without any injury. When the same experiments were tried with sea water molusca, the result was the same. If suddenly plunged into fresh water, they died. But if the sea water was slowly reduced by mixing it with a greater and greater proportion of fresh water, they may be gradually accustomed to live in fresh water. Water impregnated with sulphate of lime, or with carbonic acid, or saturated with common salt, very speedily destroyed all the molusca put into it. Hence M. Beudant thinks we can explain why no shells are found in gypsum or rock salt. Ann. de Chim. et Phys. ii. 32.

3. Mr. Dicke's curious account of the destruction of the *gasterosteus aculeatus*, or *three spined stickle-back*, by the tænia solida of Gmelin (see *Annals*, vii. 106) affords ample subject for physiological speculation.

4. Dr. Balfour's important experiments on the re-union of parts of the human body accidentally separated, are sufficiently known to my readers, as I inserted an historical account of the whole in the *Annals*, vii. 263.

5. It would be difficult to give a satisfactory explanation of the fish which make their appearance in tanks in India after rain, as I have stated in the *Annals*, viii. 70.

6. In the first number of the Journal of the Royal Institution, p. 55, Mr. Ireland has given a distinct account of the changes of the Surinam frog, from the tadpole or fish state to that of the frog. These changes were not before understood, and have occasioned a good deal of discussion among naturalists.

7. In the same Journal, i. 86, Sir Everard Home relates the case of a gentleman, aged 53, who in consequence of a paralytic stroke from which he recovered, lost the power of adjusting his eye to near distances. He could observe a pin upon the carpet at the distance of ten feet, but was unable to read the newspaper.

8. Sir Everard Home relates in the same journal a curious experiment of Mr. John Hunter, confirming an opinion universally credited by all ass keepers, namely, that an ass will not continue to give milk after she has lost the impression of her foal. He took an ass in milk that had a foal, and kept them apart every night; but had the mother milked in the morning in the presence of the foal. This was done for more than a month without there being any diminution in the morning's milk. The foal was taken away altogether, and the mother was milked instead of being sucked by the foal, particularly in the evening at the same hour at which the foal had been taken away from her, and again in the morning at the usual hour. The milk taken in the morning was always compared with that taken the morning before. But in three mornings the quantity was lessened, and the fifth morning there was hardly any; the foal was then restored to her, but she would not allow it to suck. The experiment was repeated with a similar result.

9. In the same Journal, i. 297, a case is related by Mr. Everard Brande, of a lady who swallowed from one to two tea-spoonfuls of magnesia every night for two years, and was in consequence

seized with violent pains, &c. owing to a concreted mass of the magnesia having accumulated in some portion of the larger intestines. She was restored to health in consequence of the removal of the obstruction by means of cathartic medicines.

XI. ZOOLOGY.*

The only important improvement in this branch of natural science is a new distribution of the animal kingdom by Dr. H. de Blainville, published in the Bulletin des Sciences for this year; but as it is our intention shortly to give an analysis of this ingenious system, we shall only observe that animals are divided by this learned anatomist into 25 classes.

In this place we shall correct an erroneous statement published in our translation of Cuvier's account of the proceedings of the Institute of France, viz. that *homola* is the only genus of sodophthalmous crustacea having the peduncle of the eyes composed of two joints, this structure variously modified being common to all the animals of that group. (Bull. des Sciences, 1816, p. 14.)

Risso's long expected work on the Crustacea of Nice is published in Paris, but has not yet reached this country.

ARTICLE II.

Further Observations respecting the Decomposition of the EARTHS, *and other Experiments made by burning a highly compressed Mixture of the Gaseous Constituents of* WATER. In a Letter to the Editor from Edward Daniel Clarke, LL.D. Professor of Mineralogy in the University of Cambridge, and Member of the Royal Academy of Sciences at Berlin, &c.; being a Continuation of the Article published in a former Number of this Work.

(To Dr. Thomson.)

SIR,

IN my last letter to you I mentioned an explosion; in consequence of which, my apparatus being destroyed, a temporary suspension of my experiments necessarily took place. The cause of that explosion may be now explained. Upon a careful examination of the fragments of the glass tube I then employed, and by comparing them with another which I had used before during nearly a quarter of a year, until it was reduced to a piece not exceeding $1\frac{3}{8}$ of an inch in length, it appeared that I had substituted a tube of $\frac{1}{60}$ of an inch in diameter for a tube whose diameter only equalled $\frac{1}{80}$ of an inch. The difference, indeed, is hardly perceptible to the eye, and may be considered as of little importance; but it is nearly that of *two* to *one*; for the areas of the sections of cylindrical tubes being as the squares of their diameters, the area of a tube whose diameter equals $\frac{1}{60}$ of an inch is to the area of a tube with a diameter of $\frac{1}{80}$ as 16 to 9. Yet it is extremely desirable that experiments should be made with tubes whose diameters are, at the least, equal to $\frac{1}{60}$ of an inch; because the heat is thereby rendered incomparably greater; but as the danger is also greater, it is necessary to devise some expedient, whereby, making allowance for the probability of an explosion, the operator may be protected from injury. Various methods have been proposed; such, for example, as that of having different reservoirs for the two gases; so that their union may only take place immediately preceding, or in the moment of their combustion; which does not answer; because it is difficult, if not impracticable, to measure the discharge of the two gases; allowing exactly two portions of *hydrogen gas* to enter into combination with one of *oxygen gas*; and unless this proportion be observed, the gas will not burn well. Another plan proposed was to surround the apparatus, either with folds of cloth, or with a cage of stout wire, or with a case of malleable iron; all of which have their inconveniencies, which it would be tedious to mention. The best method that I have yet tried was recommended by our Professor of Chemistry, the Rev. J. Cumming. Mr. Newman sent to me a blow-pipe constructed according to Professor Cumming's plan. It contains a small cylindrical chamber, which is to be half filled with water, through which the gas is made to pass in its passage to the jet of the apparatus. If an explosion take place, it extends only to the surface of the water, and does not communicate combustion to the compressed gas in the reservoir. Towards the bottom of the cylinder, for containing the water, there is placed a wire-gauze; and there are other trivial circumstances which tend to render the apparatus secure; but these I shall not now particularly detail, because Mr. Newman himself proposes to publish an account of this blow-pipe. Suffice it only to say, that, with all the advantages of this ingenious contrivance, an explosion will sometimes happen; and one has actually happened; my own apparatus, thus constructed, having exploded this day. If during the partial explosions which extend to the surface of the water, this fluid be driven into the reservoir, or if through any inattention in the operator the handle of the syringe be drawn out, while the stop-cock below it is open, previously to the introduction of the gas into the reservoir, then, the air in the reservoir being partially exhausted, the water will rush into it, and an explosion becomes extremely probable; nor will the wire-gauze prevent it; as it has been proved by the explosion I this day witnessed. However, this new contrivance is a very good one, provided the operator do not attempt to exhaust the reservoir of atmospheric air, and will be at the pains to listen, and to ascertain whether the water boil, owing to the passage of the gaseous bubbles, before he ventures to ignite the gas. My object is, to suggest an expedient, whereby, whatsoever explosion may happen, whether using Mr. Newman's original blow-pipe, or one of those made according to Professor Cumming's improvement, the operator may be perfectly secure from danger. Such an expedient I have adopted, since I sent my last letter to you; and, in the security it offers, I have been enabled to continue my experiments; although I have witnessed two explosions with the utmost impunity. Its simplicity may perhaps recommend it; and that my description of it may be perspicuous, it will be sent to you accompanied by a drawing. (Plate LX.)

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* For this article I am indebted to a friend.

ate IX

Fig 1

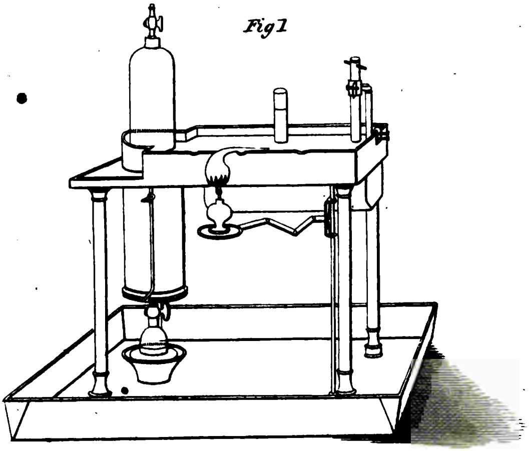


Fig 2

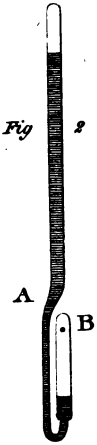
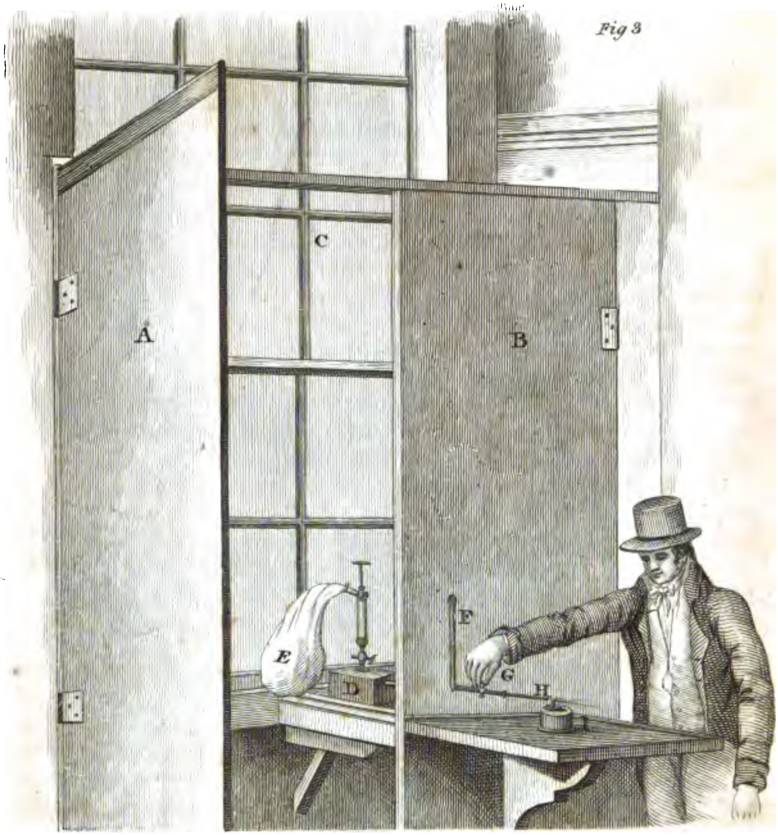


Fig 3



Rd. Banks Harraden, del.

J. S. Sculp.

This expedient consists in nothing more than in having a screen made of deal planks, which are 1¼ inch thick, and reach about 12 feet from the floor of the laboratory. It is so constructed that one half of it opens like a door; the other half remaining fixed. The blow-pipe is placed behind the half that is fixed; and a small hole is bored through this half, which is barely large enough to allow the jet and stop-cock to pass through. By means of the door, the operator has, at all times, access to the piston for compressing the gas; and when this door is closed, the gas may be ignited without a possibility of danger. If an explosion happen, the screen protects the operator. This screen may be also placed before a window; if its height and situation be such as to secure persons without from any consequences of an explosion; and in this case, as the force of the explosion acts generally in the opposite direction to that of the flame, any part of the copper box which may be driven off will escape without repercussion.

The drawing which accompanies this will show the situation of the gaseous reservoir of the blow-pipe before the door of the screen is closed upon it. (See the Plate.)

A B is the deal screen, in two parts; A being made to open; and B a fixture; before the window, C.

D represents the gaseous reservoir of the blow-pipe.

E the bladder containing the gaseous mixture for compression.

F the hand of the operator upon the stop-cock of the jet, on the outside of the screen.

G H a tube of glass, or of brass, for the jet.

I the spirit lamp for igniting the gas.

The great advantage of this screen over the plan of having cases, or covers, for the blow-pipe, is this; that the experiments are not interrupted or delayed by the necessity of removing from the apparatus the bladder and piston every time that a fresh supply of the gaseous mixture has been compressed into the reservoir. All that is required, previously to condensing the gas, is to open the door; and, previously to its ignition, to close the same, as a security from danger.

In this manner, as I have before stated, I have been enabled to continue my experiments. The new results, which I have obtained, will perhaps interest your readers; and, consistently with my former communication, I will endeavour to state them with as much brevity as the nature of the subject will admit.

Further Experiments with the ignited Gas.

1. *Sand Tubes of Drigg, in Cumberland.*—This experiment was made at the suggestion of H. Warburton, Esq. I had maintained in my Lectures, before the University, that the substance investing the interior of these tubes was not a vitrified body, but a siliceous concretion analogous to *hyalite* or *pearl-sinter*. The result of its exposure to the ignited gas has confirmed me in this opinion. Its fusion was instantaneous; and similar to the fusion of *hyalite*; leaving a bead of pure limpid glass; containing bubbles; like *rock crystal* after fusion.

2. *Carbonaceous Substance which floats on Pig Iron during its first Fusion.*—This substance, owing to its property of soiling the fingers, and to its general appearance, has been supposed to be *plumbago*. It was transmitted to me by Mr. Herschell, as a subject for trial before the ignited gas; in consequence of the request of a lady, Mrs. Lowry, from whom he received it. Dr. Wollaston, according to Mr. Herschel's information, had "considered it as being the most intractable substance by fire he had ever met with." By a letter which I have since received from Dr. Wollaston, I find that he has proved this substance to be a *carburet of manganese*; and he states that it is called *kish* by the iron masters. Mr. Lowry had, however, found some specimens of it containing $\frac{999}{1000}$ of *carbon*. When brought, *per se*, into contact with the ignited gas, a scintillation ensues resembling the sparks thrown out by the sort of firework which is called a flower-pot, but upon a smaller scale. When placed upon *charcoal* the same appearance takes place, until fusion begins, when a bead of metal is formed upon the *charcoal*; and as soon as this begins to boil, such a rapid combustion takes place that the whole of the *metal* seems to be sent forth in a volume of sparks. The bead of this metal exhibits to the file a bright *metallic* lustre like *iron*; both before and after fusion it is magnetic.

3. *Carburet of Iron, or Plumbago, from America.*—Having by me a specimen of this substance, remarkable for its purity, which was presented to me by the late Professor Tennant, I selected a small fragment, and brought it into contact with the ignited gas. Its fusion immediately ensued; being accompanied, at the same time, with that vivid scintillation which was remarked in the preceding experiment, and which denotes the combustion of *metallic* bodies; especially of *iron* and of *platinum*. No change of colour was, however, to be observed in the flame; the light, as usual, was intense. Upon examining the appearance of the *plumbago*, after fusion, its surface was covered with innumerable minute globules, some of which were a limpid and transparent glass; others a glass of a brownish hue; and the larger globules are jet black, and opake; and seem to exhibit a dark *metallic* lustre; but being so exceedingly minute, it is difficult to ascertain their real nature. They sink in *naftha*, disengaging bubbles of gas. Water produces no change in their appearance; they fall rapidly to the bottom, and remain there without alteration.

4. *Substance commonly called Gadolinite.*—Nothing is more usual among mineralogists than to see a substance exhibited under the name of *gadolinite*, which is supposed to be distinguished in its external characters from *tantalite*. But these minerals are often confounded. It is therefore necessary to premise that the substance now alluded to, which came to me from Sweden under the name of *gadolinite*, is utterly *infusible* by the common blow-pipe: consequently, according to the observations of Hausmann,* it ought rather to be considered as *tantalite*. Before the ignited gas its fusion is instantaneous; it leaves a black shining bead, which is not magnetic; and this upon the action of the file discloses a brilliant

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* See Jameson's Mineralogy, iii. 567. Edinburgh, 1816.

metallic lustre like the *metal* of *barytes*. The external appearance of the substance, after fusion, and before being rased by the file, is also like pure *barytes* that has been exposed to the same degree of temperature; that is to say, it fuses into a globular form, which is of a jet-black colour, and shines with a considerable degree of *metallic* lustre. In all probability this metal is *tantalum.*

5. *Ancient Egyptian and Roman bronze Medals.*—Having, upon a former occasion, alluded to the easy test afforded by this blow-pipe in distinguishing ancient *bronze*, from modern *brass*, and suspecting that the coins of the Romans in the *second* century might contain *zinc*, and therefore be of the latter description, I determined to submit to this test a medal of Marcus Aurelius Antoninus, and to compare its action before the ignited gas with the fusion of a *bronze* medal struck under the Ptolemies, in Egypt. There was, however, no perceptible difference; the metallic compound, in either instance, consisting of *copper* alloyed with *tin.* The fusion was tranquil, without any scintillation, or any deposit of *zinc* oxide upon the iron forceps used as a support. Afterwards, by placing the results in *nitric* acid, the *copper* was dissolved, and *tin* remained, in the form of a white precipitate; this precipitate being collected, washed, and dissolved in *muriatic* acid, afterwards precipitated *platinum* from its solution in *nitro-muriatic* acid. I had previously estimated the specific gravities of these alloys, and found them to be as follow:—

Bronze medal of the Ptolemies	8·2777
Bronze medal of Marcus Aurelius Antoninus	8·6129

6. *Alloy of the Metal of Barytes with Silver.*—I have before mentioned the appearance exhibited by this alloy. During two months it preserved its *metallic* appearance unaltered, and was so readily acted upon by the file, that I considered the *silver* as predominating, and that the *metallic* splendour, disclosed by rasing it, was mainly due to its presence; but at the expiration of the time I have mentioned, I found, to my great surprise, that the entire mass had assumed an *earthy* form, by mere exposure to atmospheric air in a warm and dry room; and that its particles, ceasing to cohere, had separated from each other; so that nothing remained of the alloy but the pulverulent appearance which had resulted from its disintegration.

7. *Vitrification of the Metals of the Earths, and some of the Semi-metals upon Charcoal.*—In all the experiments that I have made with the ignited gas where *charcoal* has been used for a support, this inexplicable property has been more or less manifested. Pure *barytes*, mixed with *soot* and *lamp-oil*, and placed within a cavity at the end of a stick of *charcoal*, instead of exhibiting the dark appearance, which during its fusion, *per se*, denotes its incipient reduction to the *metallic* state, becomes white, and assumes a vitreous aspect; but when the vitrified mass is taken out of the *charcoal*, and exposed alone to the ignited gas, fusion ensues, attended with combustion, scintillation, and the revival of the *metal.* Are we to conclude from this that the base of *charcoal* is itself *metallic?* or that the *metal* is a compound body resulting

from the union of *hydrogen* with the substance which appears to be revived in the *metallic* state? These are queries which I leave to the consideration of your readers. It would extend these observations too far if I were to enter fully upon the subject of this property in *charcoal.* Many eminent chemists, to whom I have applied for its explanation, are unable to account for it; and the chemical readers of your *Annals* are well aware that the nature of *charcoal* is, at present, too problematical, to expect a very satisfactory elucidation of all its properties.

8. *Metals of the Earths.*—With respect to the *metals* which I have obtained from *silex, barytes,* and *strontian,* and especially from the last two, the frequent revival of which to gratify the curiosity of chemical friends, has engaged almost all my subsequent experiments, I ought to mention that unless there be a sufficient body of flame, even by means of the ignited gas they cannot be obtained for want of heat. A tube with too small a diameter has been the cause of failure in some of my own experiments that were made with a view to the revival of those metals. With Newman's improved blow-pipe, using the screen I have described as a protection, I should consider failure as almost impossible. Among a great number of individuals to whom, since the month of August last, I have exhibited these *metals* in the instant of their reduction to the *metallic* state, there has not been a single instance where the slightest doubt was entertained, at the time, of their *metallic* nature. But when I have sent them to a considerable distance in *naftha,** instances have occurred in which these substances arrived at their destination without any *metallic* lustre. This happened once with respect to some brilliant *metallic* globules, obtained from the *nitrate of barytes,* which were sent to you for examination. The same event occurred when I endeavoured to transmit the *metal* of *barytes* to Dr. Wollaston. It left upon the file a white powder, and exhibited no degree of *metallic* lustre; and what rendered this the more remarkable was, that I sent to Dr. Wollaston the identical specimen which I had here exhibited to the Dean of Carlisle, once our Professor of Chemistry, in a highly *metallic* state. For the appearance of these bodies under such circumstances, I cannot be responsible; chemists will now obtain these *metals* for themselves, and in a less questionable form. Dr. Trail, of Liverpool, has repeated many of my experiments; and I am indebted to this gentleman for his communications respecting the mode in which he conducts his own experiments with the ignited gaseous mixture. Many other chemists are similarly occupied, and the powers of this extraordinary engine of chemical decomposition will soon be more generally known than they are at present.

9. *Oriental Rubies.*—Dr. Wollaston sent to me two *rubies,* with a view to ascertain whether, during fusion, they would unite into one mass. One of them had a tolerable degree of colour; the other was nearly limpid and white. Being placed upon *charcoal,*

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* It may be proper to state, once for all, that the only correct way of writing this word is with *f*, as *naftha.* In this manner it is written by the Persians; who have no *ph*; therefore *naphtha* and *naptha* are both erroneous.

their fusion was so rapid that I feared they would volatilize. They ran together into a bead, and remained in such a liquid state before the gas, that the current of it penetrated like a stream of air upon oil, when urged by a pair of bellows. The bead when examined was white and opake; all colour having disappeared. It was then again exposed to the ignited gas, and being taken from the *charcoal*, by *iron* forceps, its surface was covered with a thin flaky *metallic* substance, which came off upon the fingers, glittering like scales of the *carburet of manganese*, before mentioned. Being a third time fused, it assumed a variety of shapes, like *sapphire* during fusion. As its bulk seemed to be now diminished, the operation was concluded: the bead when cold exhibited a pale pink colour; probably owing to a small portion of *silex*. In this state it was returned to Dr. Wollaston.

10. *Reduction of Tin Oxide.*—This affords an easy and very pleasing experiment. *Wood-tin* exposed to the ignited gas communicates a beautiful blue colour, like that of violets, to the flame, which I believe has not been before noticed. If a pair of *iron* forceps be used as a support, the *iron* becomes covered with an oxide of *tin* of incomparable whiteness. The fusion is rapid; and if the *wood-tin* be placed upon *charcoal*, the *metal* is revived in a pure and malleable state.

11. *Reduction of Iron Oxide.*—In this experiment, as I had used *wood-tin* in the preceding trial, I made use of *wood-iron*; or *fibrous red hœmatite.* It was placed upon *charcoal*, and instantly fused; being reduced to a bead, which began to burn, like *iron-wire* by continuance of the heat. When cold, it exhibited *metallic* lustre to the action of the file; and resembled in all respects the *iron* obtained by the fusion of *meteoric stones*; excepting that it approached nearer to the state of *malleable iron.* The combustion of the metal is all that prevented its more perfect reduction. This may be effected by a slower process, with less vehement heat; as iron masters know that *cast-iron*, long acted upon by fire, in chimneys, sometimes becomes *malleable*.

12. *Fusion and Combustion of Platinum.*—As this test affords the only measure of the heat obtained in burning the gaseous mixture of *hydrogen* and *oxygen*, it may be proper to state that such is the increased temperature since using Newman's improved blow-pipe, constructed according to the plan recommended by Professor Cumming, that we are forced to check its operation when we wish to obtain large drops of this metal from *platinum* wire. The diameter of the jet is now so much enlarged, that the flame of the ignited gas extends more than two inches beyond the orifice; and the gas may be made to burn with a flame five or six inches in length, without danger of an explosion, by using proper caution: consequently the fusion of the platinum is so rapid that the drops fall from it, like drops of water from melting ice; and this fusion is all the while accompanied by a radiating scintillation, caused by the sparks given out by the metal during its combustion; affording a most pleasing and brilliant experiment. The largest drops which have fallen from melting platinum wire, when exposed to the utmost heat, weigh 10

grains; but we have obtained drops of metal weighing 14 grains when the current of gas is diminished so as not to let the metal run off too quickly from the wire. And by placing several globules upon a piece of charcoal, and suffering the whole force of the gas to act upon them, the metal is made to boil, and they all run together into one mass. In this manner, as a test of the heat, I have obtained a globule of platinum weighing 23 grains, which is now sent with others for your inspection.

13. *Semi-Metals.*—A few words respecting the substance called *semi-metals* will now conclude these observations. The description given in works of chemistry is to a certain extent erroneous with regard to these bodies, their colour, lustre, and hardness. It is an error, for example, that most of them become rapidly oxided by exposure to atmospheric air. Of course my remarks must be restricted to those metals which do not become volatilized by the heat of the ignited gas. I shall describe some of them as they now appear more than four months after their reduction to the *metallic* state.

Cobalt is a metal somewhat darker than *iron*, easily admitting the action of a file.

Manganese resembles the *metal* of *barytes*: and this you have also stated, as being your own opinion, respecting the latter. It is somewhat harder than *cobalt*; exhibiting a whiter colour, and a greater degree of lustre.

Tungsten, or *Scheelin.*—This metal I obtained from *wolfram.* It resembles the *magnetic iron ore* of *Lapland*; not being, however, itself *magnetic.* Upon the action of the file it discloses a brilliant *metallic* surface with a high degree of lustre.

Molybdenum, resembles *arsenical iron*; but when further reduced, and exhibited in the form of globules, it has the whiteness of the purest *silver.*

Uranium, is the hardest of all the *semi-metals.* The sharpest file will scarcely touch it. The colour and lustre of this metal resemble those of polished *iron.*

Titanium.—The exterior surface of this beautiful metal, after fusion, is of a black colour; like the *metal* of *barytes*, when obtained directly from the *earth.* It is very hard. When filed it is nearly as white as silver.

Cerium.—The appearance of this *metal* is like that of *iron.* It is very hard, and its surface after fusion is of a brownish colour.

I cannot conclude a description of these results without once more congratulating your chemical and mineralogical readers upon the powerful means of analysis which are now offered to their use; and as we are at length enabled to conduct every experiment without the slightest danger to the operator, I trust it will not be long before other results, far exceeding in their importance any that I have been fortunate enough to obtain, will give additional interest, not only to your *Annals*, but also to the sciences, towards whose advancement your labours have so materially conduced.

Cambridge, Dec. 4, 1816. EDWARD DANIEL CLARKE.

ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1817.

ARTICLE I.

Narrative of a Journey from the Village of Chamouni, in Switzerland, to the Summit of Mount Blanc, undertaken on Aug. 8, 1787. By Col. Beaufoy, F.R.S.

THE desire of ascending to the highest part of remarkably elevated land is so natural to every man, and the hope of repeating various experiments in the upper regions of the air is so inviting to those who wish well to the interests of science, that, being lately in Switzerland, I could not resist the inclination I felt to reach the summit of Mount Blanc. One of the motives, however, which prompted the attempt was much weakened by the consideration that I did not possess, and in that country could not obtain, the instruments that were requisite for many of the experiments which I was anxious to make; and the ardour of common curiosity was diminished when I learned that Dr. Paccard and his guide, who in the year 1786 had reached the supposed inaccessible summit of the hill, were not the only persons who had succeeded in the attempt; for that, five days before my arrival at the foot of the mountain, M. de Saussure, a Professor in the University of Geneva, had gained the top of the ascent. But while I was informed of the success which had attended the efforts of M. de Saussure, I was told of the difficulties and dangers that accompanied the undertaking; and was often assured, with much laborious dissuasion, that, to all the usual obstacles, the lateness of the season would add the perils of those stupendous masses of snow which are often dislodged from the steeps of the mountain, together with the hazard of those frightful chasms which present immeasurable gulfs to the steps of the traveller, and the width of which was hourly increasing. M. Bourrit, whose name has often been announced to the world by a variety of tracts, and by many excellent drawings, confirmed the account, and assured me that he himself had made the attempt on the next day to that on which M. de Saussure descended, but was obliged, as on many former occasions, to abandon the enterprise. Having formed my resolution, I sent to the different cottagers of the vale of Chamouni, from the skirts of which the mountain takes its rise, to inquire if any of them were willing to go with me as my assistants and my guides, and had soon the satisfaction to find that 10 were ready to accept the proposal. I engaged them all. Having announced to them my intention of setting out the next morning, I divided among them provisions for three days, together with a kettle, a chaffing dish, a quantity of charcoal, a pair of bellows, a couple of blankets, a long rope, a hatchet, and a ladder, which formed the stores that were requisite for the journey. After a night of much solicitude, lest the summit of Mount Blanc should be covered with clouds, in which case the guides would have refused the undertaking as impracticable, I rose at five in the morning, and saw, with great satisfaction, that the mountain was free from vapour, and that the sky was every where serene. My dress was a white flannel jacket without any shirt beneath, and white linen trowsers without drawers. The dress was white that the sunbeams might be thrown off; and it was loose, that the limbs might be unconfined. Besides a pole for walking, I carried with me cramp irons for the heels of my shoes, by means of which the hold of the frozen snow is firm, and in steep ascents the poise of the body is preserved. My guides being at length assembled, each with his allotted burthen; and one of them, a fellow of great bodily strength, and great vigour of mind, Michael Cachet by name, who had accompanied M. de Saussure, having desired to take the lead, we ranged ourselves in a line, and at seven o'clock, in the midst of the wives, and children, and friends, of my companions, and indeed of the whole village of Chamouni, we began our march. The end of the first hour brought us to the Glaciere des Boissons, at which place the rapid ascent of the mountain first begins, and from which, pursuing our course along the edge of the rocks that form the eastern side of this frozen lake, we arrived in four hours more at the second glaciere, called the Glaciere de la Coté. Here, by the side of a stream of water which the melting of the snow had formed, we sat down to a short repast. To this place the journey is neither remarkably laborious, nor exposed to danger, except that name should be given to the trifling hazard that arises from the stones and loose pieces of the broken rock which the goats, in leaping from one projection to another, occasionally throw down. Our dinner being finished, we fixed our cramp-irons to our shoes, and began to cross the glaciere; but we had not proceeded far when we discovered that the frozen snow which lay in the ridges between the waves of ice, often concealed, with a covering of uncertain strength, the fathomless chasms which traverse this solid sea; yet the danger was soon in a great degree removed by the expedient of tying ourselves together with our long rope, which being fastened at proper distances to our waists, secured from the principal hazard such as might fall within the opening of the gulf. Trusting to the same precaution, we also crossed upon our ladder without apprehension such of the chasms as were exposed to view; and, sometimes stopping in the middle of the ladder, looked down in safety upon an abyss which baffled the reach of vision, and from which the sound of the masses of ice that we repeatedly let fall in no instance ascended to the ear. In some places we were obliged to cut footsteps with our hatchet; yet, on the whole, the difficulties were far from great; for in two hours and a half we had passed the glaciere. We now, with more ease, and much more expedition, pursued our way, having only snow to cross, and in two hours arrived at a hut which had been erected in the year 1786 by the order, and at the expense, of M. de Saussure. The hut was situated on the eastern side of a rock which had all the appearance of being rotten with age, and which in fact was in a state of such complete decay, that, on my return the next evening, I saw scattered on the snow many tons of its fragments, which had fallen in my absence; but the ruin was not on the side on which the hut was built. Immediately on our arrival, which was at five in the afternoon, the guides began to empty the hut of its snow; and at seven we sat down to eat; but our stomachs had little relish for food, and felt a particular distaste for wine and spirits. Water, which we obtained by melting snow in a kettle, was the only palatable drink. Some of the guides complained of a heavy disheartening sickness; and my Swiss servant, who had accompanied me at his own request, was seized with excessive vomiting, and the pains of the severest headach. But from these complaints, which apparently arose from the extreme lightness of the air in those elevated regions, I myself and some of the guides were free, except, as before observed, that we had little appetite for food, and a strong aversion to the taste of spirituous liquors. We now prepared for rest; on which two of the guides, preferring the open air, threw themselves down at the entrance of the hut, and slept upon the rock. I too was desirous of sleep; but my thoughts were troubled with the apprehension that, although I had now completed one half of the road, the vapours might collect on the summit of the mountain, and frustrate all my hopes. Or if at any time the rest I wished for came, my repose was soon disturbed by the noise of the masses of snow which were loosened by the wind from the heights around me, and which, accumulating in bulk as they rolled, tumbled at length from the precipices into the vales below, and produced upon the ear the effect of redoubled bursts of thunder. At two o'clock I threw aside my blankets, and went out of the hut to observe the appearance of the heavens. The stars shone with a lustre that far exceeded the brightness which they exhibit when seen from the usual level; and had so little tremor in their light, as to leave no doubt on my mind that, if viewed from the summit of the mountain, they would have appeared as fixed points. How improved in those altitudes would be the aids which the telescope gives to vision; indeed, the clearness of the air was such as led me to think that Jupiter's satellites might be distinguished by the naked eye; and had he not been in the neighbourhood of the moon, I might possibly have succeeded. He continued distinctly visible for several hours after the sun was risen, and did not wholly disappear till almost eight. At the time I rose, my thermometer, which was on Fahrenheit's scale, and which I had hung on the side of the rock without the hut, was 8° below the freezing point. Impatient to proceed, and having ordered a large quantity of snow to be melted, I filled a small cask with water for my own use, and at three o'clock we left the hut. Our route was across the snow; but the chasms which the ice beneath had formed, though less numerous than those that we had passed on the preceding day, embarrassed our ascent. One in particular had opened so much in the few days that intervened between M. de Saussure's expedition and our own, as for the time to bar the hope of any further progress; but at length, after having wandered with much anxiety along its bank, I found a place which I hoped the ladder was sufficiently long to cross. The ladder was accordingly laid down, and was seen to rest upon the opposite edge, but its bearing did not exceed an inch on either side. We now considered that, should we pass the chasm, and should its opening, which had enlarged so much in the course of a few preceding days, increase in the least degree before the time of our descent, no chance of return remained. We also considered that, if the clouds which so often envelope the hill should rise, the hope of finding, amidst the thick fog, our way back to this only place in which the gulf, even in its present state, was passable, was little less than desperate. Yet, after a moment's pause, the guides consented to go with me, and we crossed the chasm. We had not proceeded far when the thirst, which, since our arrival in the upper regions of the air, had been always troublesome, became almost intolerable. No sooner had I drank than the thirst returned, and in a few minutes my throat became perfectly dry. Again I had recourse to the water, and again my throat was parched. The air itself was thirsty; its extreme dryness had robbed my body of its moisture. Though continually drinking, the quantity of my urine was almost nothing; and of the little there was, the colour was extremely deep. The guides were equally affected. Wine they would not taste; but the moment my back was turned, their mouths were eagerly applied to my cask of water. Yet we continued to proceed till seven o'clock; when, having passed the place where M. de Saussure, who was provided with a tent, had slept the second night, we sat down to breakfast. All this time the thermometer was 4° below the freezing point. We were now at the foot of Mount Blanc itself; for, though it is usual to apply that term to the whole assemblage of several successive mountains, yet the name properly belongs only to a small mountain of pyramidal form that rises from a narrow plain which at all times is covered with snow. Here the thinness of the atmosphere began to affect my head with a dull and heavy pain. I also found, to my great surprise, an acute sensation of pain, very different from that of weariness, immediately above my knees. Having finished our repast, we pursued our journey, and soon arrived at a chasm which could not have existed many days, for it was not formed at the time of M. de Saussure's ascent. Misled by this last circumstance, for we concluded that, as he had seen no rents whatever from the time that he passed the place where he slept the second night, none were likely to be formed, we had left our ladder about a league behind; but as the chasm was far from wide, we passed it on the poles that we used for walking; an expedient which suggested to me that the length of our ladder might be easily increased by the addition of several poles laid parallel and fastened to its end; and that the hazard of finding our retreat cut off from the enlargement of the chasms might by this means be materially diminished. At this place I had an opportunity of measuring the height of the snow which had fallen during the preceding winter, and which was distinguished by its superior whiteness from that of the former year. I found it to be five feet. The snow of each particular year appeared as a separate stratum; that which was more than a twelvemonth old was perfect ice; while that of the last winter was fast approaching to a similar state. At length, after a difficult ascent, which lay among precipices, and during which we were often obliged to employ the hatchet in making a footing for our feet, we reached and reposed ourselves upon a narrow flat which is the last of three from the foot of the small mountain, and which, according to M. de Saussure, is but 150 fathoms below the level of the summit. Upon this platform I found a beautiful dead butterfly, the only appearance which, from the time I entered on the snow, I had seen of any animal. The pernicious effects of the thinness of the air were now evident on us all; a desire, almost irresistible, of sleep came on. My spirits had left me; sometimes indifferent as to the event, I wished to lie down; at others, I blamed myself for the expedition; and, though just at the summit, had thoughts of turning back, without accomplishing my purpose. Of my guides many were in a worse situation; for, exhausted by excessive vomiting, they seemed to have lost all strength, both of mind and body. But shame at length came to our relief. I drank the last pint of water that was left, and found myself amazingly refreshed. Yet the pain in my knees had increased so much, that at the end of every 20 or 30 paces I was obliged to rest till its sharpness was abated. My lungs with difficulty performed their office, and my heart was affected with violent palpitation. At last, however, but with a sort of apathy which scarcely admitted the sense of joy, we reached the summit of the mountain; when six of my guides, and with them my servant, threw themselves on their faces, and where immediately asleep. I envied them their repose; but my anxiety to obtain a good observation for the latitude subdued my wishes for indulgence. The time of my arrival was half an hour after ten; so that the hours which had elapsed from our departure from Chamouni were only 27½, 10 of which we had passed in the hut. The summit of the hill is formed of snow, which spreads into a sort of plain which is much wider from E. to W. than from N. to S., and in its greatest width is perhaps 30 yards. The snow is every where hard, and in many places is covered with a sheet of ice. When the spectator begins to look round him from this elevated height, a confused impression of immensity is the first effect produced upon his mind; but the blue colour, deep almost to blackness, of the canopy above him soon arrests his attention. He next surveys the mountains; many of which, from the clearness of the air, are to his eye within a stone's throw from him; and even those of Lombardy (one of which appears of an altitude but little inferior to that of Mount Blanc) seem to approach his neighbourhood: while on the other side the vale of Chamouni glittering with the sunbeams is to the view directly below his feet, and affects his head with giddiness. On the other hand, all objects of which the distance is great, and the level low, are hid from his eye by the blue vapour which intervenes, and through which I could not discern the Lake of Geneva, though at the height of 15,700 English feet, which, according to M. de Saussure, was the level on which I stood, even the Mediterranean Sea must have been within the line of vision. The air was still; and the day so remarkably fine, that I could not discover in any part of the heavens the appearance of a single cloud. As the time of the sun passing the meridian now approached, I prepared to take my observation. I had with me an admirable Hadley's sextant, and an artificial horizon, and I corrected the mean refraction of the sun's rays. Thus I was enabled to ascertain with accuracy that the latitude of the summit of Mount Blanc is 45° 49′ 59″ North.

I now proceeded to such other observations as the few instruments which I had brought permitted me to make. At twelve o'clock the mercury in the thermometer stood at 38° in the shade; at Chamouni, at the same hour, it stood when in the shade at 78°. I tried the effect of a burning glass on paper, and on a piece of wood, which I had brought with me for the purpose, and found (contrary, I believe, to the generally received opinion) that its power was much greater than in the lower regions of the air. Having continued two hours on the summit of the mountain, I began my descent at half an hour after twelve. I found that, short as my absence had been, many new rents were opened, and that several of those which I had passed in my ascent were become considerably wider. In less than six hours we arrived at the hut in which we had slept the evening before, and should have proceeded much further down the mountain had we not been afraid of passing the Glaciere de la Coté at the close of the day, when the snow, from the effect of the sunbeams, was extremely rotten. Our evening's repast being finished, I was soon asleep; but in a few hours I was awakened with a tormenting pain in my face and eyes. My face was one continued blister, and my eyes I was unable to open; nor was I without apprehensions of losing my sight for ever, till my guides told me that if I had condescended to have taken their advice of wearing, as they did, a mask of black crape, the accident would not have befallen me, but that a few days would perfectly restore the use of my eyes. After I had bathed them with warm water for half an hour, I found to my great satisfaction that I could open them a little, on which I determined upon an instant departure, that I might cross the Glaciere de la Coté before the sun was risen sufficiently high for its beams to be strongly reflected from the snow. But unluckily the sun was already above the horizon; so that the pain of forcing open my eyes in the bright sunshine, in order to avoid the chasms, and other hazards of my way, rendered my return more irksome than my ascent. Fortunately one of the guides, soon after I had passed the glaciere, picked up in the snow a pair of green spectacles, which M. Bourrit had lost, and which gave me wonderful relief.

At eleven o'clock on Aug. 10, after an absence of 52 hours, of which 20 were passed in the hut, I returned again to the village of Chamouni. From the want of instruments (the scale of the barometers I had being graduated no lower than 20 inches, which was not sufficiently extended) the observations I made were but few. Yet the effects which the air in the heights I visited produced on the human body may not perhaps be considered as altogether uninteresting, nor will the proof I made of the power of the lens on the summit of Mount Blanc, if confirmed by future experiments, be regarded as of no account in the theories of light and heat. At any rate, the having determined the latitude of Mount Blanc may assist in some particulars the observations of such persons as shall visit it in future; and the knowledge which my journey has afforded, in addition to that which is furnished by M. de Saussure, may facilitate the ascent of those who, with proper instruments, may wish to make in that elevated level experiments in natural philosophy.*

ARTICLE II.

On the Acids contained in the Juice of the Stems of Rhubarb.

By M. Donovan, Esq.

DURING my investigation of the nature and combinations of the sorbic acid, I had occasion to examine a great variety of vegetable juices: amongst the rest, that of the rhubard plant occupied my attention.

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* As the summit of Mount Blanc bears from Neuchatel by the compass 20° 54′ 07″ W., by using the difference of latitude and the true bearing, the longitude in space is 3′ 10″ W. of Neuchatel, and consequently 7° 6′ 50″ E. from Greenwich. (See *Annals of Philosophy*, v. 368.)

Mr. Henderson has lately examined this juice; and he announces that he has discovered in it a new and peculiar acid, which he calls the rheumic.

There are some facts with which I am acquainted, and some objections to the conclusions drawn by that gentleman, that seemed necessary to be considered before the rheumic acid can be admitted as a distinct substance. These I shall, therefore, state.

In my experiments I found the predominant acid in the stalks of rhubarb to be malic, but there was also a quantity of the sorbic present. No other acid manifested itself; but as I was not investigating with the design of discovering a new one in the plant, I do not pretend to say that it contained no other.

Of the presence of these acids, Mr. Henderson does not appear to have been aware; but he found reason to suppose that citric acid is one of the component parts of the juice. When, therefore, he saturated the acid juice with lime, he obtained malate, sorbate, and citrate of lime, all of which are insoluble salts: and when this powder was acted on by sulphuric acid, the result was a mixture of sorbic, malic, and citric acids.

When the juice was saturated with chalk, a supermalate of lime was formed; but this being soluble, it might have been washed away during the edulcoration of the precipitate.

Beside the acids naturally contained in the juice, there are others introduced by the process. The mixed salts of lime, already noticed, were decomposed by sulphuric acid, which, as during the subsequent evaporation, it charred the vegetable portion, must have been in excess. Hence sulphuric acid, along with those already mentioned, would adulterate the product. During the charring, a quantity of acetic acid must also have been produced.

By employing the other method proposed by Mr. Henderson of combining the acid juice with lead, and acting on the compound with nitric acid, we give origin to new impurities, such as nitric, oxalic, and perhaps other vegetable acids.

Thus in the resulting fluid obtained by the proposed processes there may be malic, sorbic, citric,* sulphuric, acetic, nitric, and oxalic acids. And from the great solubility of the crystals supposed to be rheumic acid, it appears that they could not be formed unless in a highly concentrated solution. Hence recrystallization would not entirely exclude the adulterating acids; and some of them would even be present in the solid form.

Several salts formed during this process would still further inquinate the substance supposed to be the pure acid. Thus malate, sorbate, citrate, and sulphate of lime, are soluble in the acids present in the resulting liquor, and might be obtained by evaporation in a crystalline form. Other salts might also have been present. Mr. Henderson no doubt satisfied himself that these acid crystals could not have been those which he considered as the rheumic acid.

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* I have no other grounds for supposing the presence of citric acid than the experiment stated by Mr. Henderson.

I examined the juice of the stalks of rhubarb within the last month. The sorbic acid had disappeared on account of the lateness of the season; the malic was present, but I could find no other. I do not, however, hence presume to offer any thing opposing the statements of Mr. Henderson. The rheumic acid, like the sorbic, might have disappeared. It is apparent that Mr. Henderson obtained malate of lime in his process; for he found that even when the juice contained much more chalk than it could dissolve, the supernatant liquor still reddened litmus. And taking every thing into consideration, it is plain that as the compounds, from the properties of which the peculiar nature of the new acid has been inferred, were formed by means of a mixture of several acids, they cannot be looked on as decisive in the question.

Mr. Henderson conceives that the new acid exists in the plant in combination with ammonia. But it should be considered that extractive matter and lime were presented to each other, and that by the mutual action of these substances ammonia is always produced.

These observations I have been induced to make, as the ingenious and candid author of the paper on which I comment acknowledges imperfections in his process, and expresses a wish that some other person might assist him in the examination of this subject. The preceding might probably be of some advantage.

ARTICLE III.

On the Composition of the Topaz; the Separation of Silica and Oxide of Tantalum; and some further Experiments on the Composition of Organic Bodies. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

(To Dr. Thomson.)

SIR, *Stockholm, Dec. 2, 1816.*

In the number of the *Annals of Philosophy* for October a very celebrated mineralogist, Mr. Gregor, has communicated some experiments on the composition of the *topaz*. He has drawn as a conclusion that this stone contains potash. I have made many experiments, as you know, on this mineral; and I consider myself as having ascertained its true chemical constitution. At the same time I did not try to discover alkali in it; because in my analyses of it, though often repeated, I never experienced any other loss than that which is almost unavoidable in experiments of that nature. After reading Mr. Gregor's letter, I thought it necessary to make a new analysis of the topaz, directing my principal attention to the extraction of potash from it.

I pulverized in an agate mortar a fine crystal of yellow topaz from Brazil, and exposed to a red heat a mixture of three grammes ($46\frac{1}{3}$ grains) of it with 12 grammes of carbonate of barytes. The mass was then put into a platinum cup, and treated with muriatic acid, which dissolved it without the disengagement of any carbonic acid, leaving no other residue but pure silica. Into the solution I poured sulphuric acid as long as any precipitate fell, and even added an excess of that acid, and then evaporated the liquid till a portion of this excess was driven off. The residue was digested with water for 24 hours, and the liquid then precipitated by carbonate of ammonia added in excess. The filtrated liquid was evaporated to dryness. The sulphate of ammonia thus obtained was put into a platinum crucible, exactly weighed, and exposed to the heat of a spirit of wine lamp. The ammoniacal salt was volatilized, and left upon the sides of the crucible reddish stains, besides a trace of a saline matter at the bottom. The crucible had gained only six milligrammes (0·0924 grain). I poured water into it. The red stains were not attacked; but the saline matter was dissolved. The small quantity of liquid thus obtained was divided into two portions. The one was evaporated to dryness, and the saline residue examined by the microscope. The crystals were irregular, and confounded together; and none of them bore any resemblance to sulphate of potash. When dissolved in water, and mixed with tartaric acid, I could perceive no trace of supertartrate of potash. The other portion, being mixed with ammonia, let fall alumina. Hence it is evident that the salt under examination, at least the greatest part of it, was sulphate of alumina. I shall not determine whether it might contain any trace of alum, as it is evident that this can have no influence.

I consider this experiment as a decisive proof that potash does not belong to the chemical constitution of the topaz, and that, if we find traces of that alkali in topazes of certain districts, it can only be considered as one of those foreign substances with which minerals are so frequently mixed. It can scarcely be doubted that topazes exist mixed with traces of felspar, just as there are crystals of nitre mixed with common salt, without our drawing as a conclusion from this that muriatic acid is a constituent part of crystallized saltpetre.

You will permit me, perhaps, to add an observation with respect to the existence of these traces of sulphate of alumina, although the liquid had been precipitated by an excess of carbonate of ammonia. The cause of it is, that alumina is not absolutely insoluble in ammonia. Caustic ammonia dissolves a considerable quantity of it. A gros (72 grains) of caustic ammonia dissolves a large piece of alum without leaving any residue, and the alumina dissolved by ammonia is not precipitated till after a long continued ebullition. Carbonate of ammonia, on the other hand, dissolves so little alumina, that, unless we add a great excess of it, the dissolved portion may be neglected altogether. A great excess, however, dissolves a little, though the portion be still small; and this was the cause of the phenomenon which occurred in my analysis. From this we may judge to what mistakes we are liable when we employ salammoniac to precipitate alumina from caustic potash.

One of your correspondents has asked the method of separating silica from oxide of tantalum. The separation of these two bodies is much more difficult than would be believed at first. The oxide of tantalum treated by an alkali is soluble in acids, just as happens to the silica; while a portion of the silica, on the other hand, remains undissolved in combination with the oxide of tantalum. You will see in my analyses of the yttro-tantalites how soluble the oxide of tantalum is in acids. The only method by which I conceive these two substances can be separated is the following:—Fuse the oxide of tantalum with bisulphate of potash. Wash off the soluble portion of this mixture by means of boiling water. Then dissolve the oxide of tantalum by means of quadroxalate of potash, which must be boiled for a considerable time over it. The silica remains undissolved, though still retaining a little oxide of tantalum.

I shall here make a small addition to the analyses of organic bodies which I communicated to you some years ago; though I have not been able hitherto to prosecute the subject much further. Formic acid, neutralized by the oxide of lead, gives a formiate composed of

Formic acid	25·12	100
Oxide of lead	79·88	298·1

Hence the capacity of that acid for saturation is 21·314. Analysed in the same way as the other acids were, 100 parts of it were found to consist of

Hydrogen	2·807
Carbon	32·970
Oxygen	64·223

These numbers correspond very nearly with the formula $2\,H + 2\,C + 3\,O$, which supposes the composition as follows:—

Hydrogen	2·84
Carbon	32·40
Oxygen	64·76

There is in my result a small excess of carbon amounting to about half a per cent. I propose to repeat this experiment as soon as I have leisure to recommence my investigations on the composition of organic bodies. The comparison of the composition of the following acids is curious enough:—

	O		C		H
Oxalic acid	3	+	2	+	$\frac{1}{8}$
Formic acid...............	3	+	2	+	2
Succinic acid	3	+	4	+	4
Acetic acid	3	+	4	+	6
Gallic acid	3	+	6	+	6
Benzoic acid	3	+	15	+	12

You will excuse me for not having adopted your correction of my analysis of oxalic acid. Let it be shown by experiment that oxalic acid gives more water when decomposed than I found, and I will give up the point immediately. You have no doubt observed that other chemists have gone to the other extremity, that of denying the existence of hydrogen altogether in oxalic acid, and of considering it as *carbonous acid.* But that idea does not appear probable to me. I am, &c.

JACOB BERZELIUS.

ARTICLE IV.

To find the Heights of Mountains with the Barometer, by Means of a Table of Compound Interest. By Adam Anderson, Esq. Rector of the Perth Academy.

(To Dr. Thomson.)

DEAR SIR,

The usual method of finding the altitudes of mountains with the barometer, by means of a table of logarithms, being founded on the principle that the density of the air decreases in a geometrical ratio, while the corresponding heights increase in an arithmetical progression, or, in other words, that the heights are the logarithms of the densities, it occurred to me that a table of compound interest might be conveniently substituted for a table of logarithms when the latter could not be procured; more especially as all the amounts necessary may be obtained in a few minutes by the actual involution of the amount of 1*l.* for a year.

A table of the amount of 1*l.* compound interest is obviously a system of logarithms the base of which is the amount of 1*l.* for a year, and the successive years a series of logarithms the numbers corresponding to which are the opposite amounts. Hence the expression for the difference of altitude between two stations may easily be reduced from Brigg's logarithms to what may be called *interest* logarithms, by help of the well-known property that in different systems the logarithms of the same number are inversely as the logarithms of the bases of the systems, the latter being taken according to any system whatever.

Let H, therefore, denote the difference of altitude in fathoms between two stations, and let D and *d* be the densities, or lengths of the barometrical columns, both being corrected for temperature; also let L represent Brigg's logarithms, and *l* the *interest* logarithms. Then by the ordinary formula, if the temperature of the air be disregarded,

$$H = 10{,}000\ [L . D - L . d].$$

But $L . D : l . D ::$ log. amount 1*l.* for a year : log. 10.
Which, if the rate be taken at five per cent., and the logarithms of the two bases, according to the common system, becomes
$L . D : l . D :: L . 1{\cdot}05 : L . 10$
That is, $L . D : l . D :: {\cdot}0211893 : 1$
And $L . D = {\cdot}0211893 \times l . D$
In like manner, $L . d = {\cdot}0211893 \times l . d$
Therefore, $H = 10{,}000\ [L . D - L . d] = 10{,}000 \times {\cdot}0211893$
$[l . D - l . d] = 211{\cdot}893\ [l . D - l . d] = 212\ [l . D - l . d]$
nearly (1).

The last expression may be reduced to a form more convenient for calculation, by assuming $H = 212\ l . \left(\frac{D}{d}\right)$ (2).

The constant quantity 212, being the same as the number for the boiling point of Fahrenheit's scale, is as easily recollected as the number 10,000 in the ordinary formula, when Brigg's logarithms are employed.

Having thus deduced a general expression for H in terms of interest logarithms, I shall now illustrate the formula by example. Before doing this, however, it will be necessary to subjoin the amounts of 1*l.* for a few successive years as a groundwork for calculation. If the first formula be used, these amounts, with the years corresponding to them, may be taken from any continuous part of the table; for since $L . \left(\frac{n\,D}{n\,d}\right) = L . \left(\frac{D}{d}\right) = L . D - L . d$, the number *n* being any number, integral or fractional, it is evident that if D and *d* are too great to be found in the table, as will generally be the case, it will answer equally well to take corresponding parts of them. This reduces the range of amounts necessary to very narrow limits. As the height of the mercurial column at the lower station is seldom the double of its height at the upper, it will be sufficient, in most cases, to make the table extend from the amount for one year to the amount for 15 years. The following table, however, is extended as far as the amount for 20 years, and will answer for finding any altitude not exceeding 25,000 feet.

Amounts of 1*l. at five per Cent. Compound Interest.*

Years.	Amounts.	Years.	Amounts.
1	1·05	11	1·71034
2	1·1025	12	1·79586
3	1·15762	13	1·88565
4	1·21551	14	1·97993
5	1·27628	15	2·07893
6	1·34010	16	2·18287
7	1·40710	17	2·29202
8	1·47746	18	2·40662
9	1·55133	19	2·52695
10	1·62895	20	2·65330

To make the example as simple as possible, it may be proper, in the first place, to suppose a case in which corresponding parts of the numbers expressing the heights of the barometrical columns, are found among the amounts exactly. Thus, let the height of the barometer at the lower station be 28·142 in., and at the upper 21 in.; if each be divided by 20, we obtain 1·4071 and 1·05, which are the exact amounts for seven years and one year; but these latter numbers being the logarithms of the former,

$$H = 212\,[l\,.\,D - l\,.\,d] = 212 \times \overline{7 - 1} = 1272 \text{ fathoms.}$$

If formula (2) be used, which, in most cases, will be more convenient,

$$H = 212 \times l\left(\frac{D}{d}\right) = 212 \times l\left(\frac{28\cdot142}{21}\right) = 212 \times l\,.\,(1\cdot3401)$$

$= 212 \times 6 = 1272$ fathoms. The result by Brigg's logarithms is 1271·357 fathoms.

Next, let a case be taken in which neither the barometrical heights themselves, nor corresponding parts of them, can be found in the table exactly: thus, employing the data of the above example, and taking $\frac{1}{12}$th instead of $\frac{1}{20}$th of the heights of the barometrical columns, we obtain 2·34517 and 1·75. As neither of these numbers is found among the amounts, we must take the years corresponding to the next lowest for both, and then find a proportional part for the excess of each. The next lowest among the amounts to 2·34517 is 2·29202, the amount for 17 years, and the difference is ·05315; also the excess of the next highest, viz. 2·40662, above 2·29202 is ·1146,

And ·1146 : ·05315 :: 1 : ·4638.

Hence 2·34517 of amount corresponds to 17·46378 years.

In like manner, it will be found that 1·75 amount corresponds to 11·46374 years.

Therefore $H = 212 \times \overline{17{\cdot}46378 - 11{\cdot}46374} = 212 \times 6{\cdot}00004 = 1272{\cdot}008$ fathoms.

The difference between the two results, which is very inconsiderable, arises from the proportional parts being calculated on the supposition that the amounts, as well as the logarithms, or years corresponding to them, increase in an arithmetical progression. This, though in no case strictly true, will seldom lead to an error of half a fathom in the altitude, a degree of accuracy which is certainly greatly within the limits, furnished by the physical data of the problem. A practical rule for finding the height in fathoms may be given in the following words:—

Divide the height of the barometrical column at the lower station by its height at the upper station, extending the division as far as the data admit; search for the quotient among the amounts of 1*l.* at five per cent. compound interest, and the years corresponding to it being multiplied by 212, will be the difference of altitude between the two stations in fathoms.

On the whole, though the method which I have proposed for calculating heights by barometrical measurements must often be more tedious and intricate than the ordinary method by logarithms, it has this important advantage, that all the elements of the calculation may be laid down on a small slip of paper, or obtained in a few minutes by the involution of 1·05. It may, therefore, be regarded as a convenient substitute, in certain cases, for the ordinary method; and in this point of view alone I have been induced to submit it to your consideration.

I am, dear Sir, with much regard and esteem,

Yours sincerely,

Perth Academy, Nov. 21, 1816. ADAM ANDERSON.

ARTICLE V.

Curious Experiments on boiling Tar. By Richard Davenport, Esq.

(To Dr. Thomson.)

DEAR SIR,

IF you think the following detail of a curious fact will be interesting to your readers, it is at your service. Some know, and many probably have heard without believing, and to others it will be quite new to hear, that a man can dip his hand into boiling tar without suffering. I have met with gentlemen who had seen it done many years ago; but I never, till very lately, had an opportunity of being myself an eye witness of the experiment; and I had supposed that the instances were not many, and that the escaping unhurt was owing to peculiar thickness, or to some other quality in the skin, or perhaps to some dexterous management, which either carried in air, or repelled the tar for a short time, as common liquids are repelled from surfaces covered with certain dusts.

I was lately taken by a friend through the King's Dockyard at Chatham, and fortunately saw the operation of tarring ropes going on. The cauldron or cistern in which the tar is heated is, I think, about six feet long and four feet wide, and 2¼ feet deep, of an oval form. The rope-strands enter at one end, and pass out at the other end, having been in their passage pressed deep under the surface. The fire is underneath, and the whole appeared in a state of ebullition.

I asked the men if they had ever seen any one dip his hand into tar in that state. One of them immediately drew up his coat sleeve and dipped his hand and wrist in, bringing out fluid tar, and pouring it off from his hand as from a ladle. The tar remained in complete contact with his skin, and he wiped it off with tow. Satisfied that there was no deception, and seeing no room for the exercise of any dexterity by which the heat could be avoided, I asked him whether there was any danger in my trying it myself. Being assured there was not, I dipped in the entire length of my fore finger, and moved it about a short time before the heat became inconvenient. I began to think that the tar was only kept to a thin liquid state by a heat much below the boiling point. I asked whether they knew what the heat was. They answered that their thermometer was broken, and a new one expected; but that their orders were to keep it between 210° and 220°, and as near as they could to the lower of the two points. I procured a bottle (a green wine-pint), and putting a small quantity of water in it, suspended it in the tar. I waited till I was tired of watching for the boiling of the water, but cannot say how long. On pouring some out, it was barely hot. I then concluded that the heat of the tar was much below that of boiling water, and that the graduation of their thermometer was different from that of Fahrenheit, and attributed the apparent ebullition to the escape of air from the heated rope-strands.

A few days afterwards, however, I had an opportunity of seeing the operation again, and a new thermometer was obtained, graduated according to Fahrenheit, marked at 212° "water boils," and a space left to mark "tar boils," against the degree to be found on trial. The apparatus for the thermometer consisted of a copper tube, of about 2½ or 3 inches diameter, closed at bottom. It was fixed upright near the middle of the cauldron, plunged two feet into it, and projecting nearly two feet out of it. The thermometer was suspended low in the copper tube, and a thin cap was put on the upper end. Nearly a quarter of an hour passed before the thermometer ceased to rise (it having been frequently examined), and it then stood at 180°, and could not be raised higher. I saw the tar evidently boil round the edges of the cauldron. A phial of water which I had left for some time suspended in it seemed to boil; but as the tar and scum had got into it, I could not easily ascertain this point. Feeling the top of the copper tube, and finding it quite cool, I was satisfied that there was much loss of heat from the tube, and of course from the included air; and the thermometer could only indicate the heat of the air. I obtained permission to bend back the wood at the joint, and I plunged the naked bulb into the tar. It rose *slowly* to 220°. It was evident that their mode of applying the thermometer was quite ineffectual, and I suggested to them an easy way of correcting it, viz. to keep water in the copper tube, which would give the true heat of the tar; and as they wish to keep the heat down to 210°, the water in a long covered tube would not evaporate very fast; a method which I believe they will try.

Once or twice during the operation, when the scum had been taken off of the surface of the liquid tar, I poured some water upon it from a phial. It spread, and was lost instantaneously, without any starting or hissing, as when water falls on hot oil; it lying, in this case, on the top of the hot fluid. It may be observed that the temperature of the tar in the process of tarring ropes varies, owing to the frequent addition of cold tar to supply the loss of that which is imbibed and carried off by the ropes, and by the evaporation.

When I saw the thermometer at 220°, and the tar thoroughly boiling, I again plunged in my finger, and moved it backwards and forwards, making three oscillations of six or eight inches. On repeating this motion afterwards, and considering it with my stop-watch, I think it occupied between two and three seconds of time. The tar certainly felt hotter to the skin than when I had tried it before; and it would not have been pleasant to prolong the time of immersion; but the heat did not rise to a painful degree, and it ceased immediately on taking out my finger, although the tar adhered to the skin just as any liquid of similar viscidity would. I suffered no inconvenience except a slight brownish stain, which disappeared after the common washing of two days.

When we endeavour to account for the absence of pain and injury to the skin in the experiment, when a far less degree of heat in other liquids would produce serious scalds, it appears from the above described facts that the immediate cause is the *slowness* with which the tar communicates its heat. It is evident that it is possessed of free heat of temperature superior to that of boiling water; for the thermometer indicates 220°; but the thermometer rose *very slowly* from 180° to that point. The heat is greater than the hand can bear if continued long in the liquid, but it remains in it for some seconds of time before it is felt. The phial of water which I suspended in the boiling tar did (I have little doubt) actually boil, but I know it remained in for a great length of time before it acquired its highest temperature.

But now what is the cause of this extraordinary slowness of communication of heat of temperature from a dense heated liquid fluid to bodies immersed in it? On this I should be glad to see the opinion of others better qualified than myself. All I can offer is the supposition that the very abundant volatile (dry looking and dry feeling) vapour evolved (very pungent both to the eyes and nostrils) rapidly carrying off the caloric in a latent state, intervenes between the tar and the skin, and prevents the more rapid communication; and that when the hand is withdrawn, and the hot tar adhering, the rapidity with which this vapour is evolved from the surface exposed to the air cools it immediately.

While I was conversing in the place, I heard it said that if a man put his hand into the cauldron with a glove on, he would be dreadfully burnt; but no one could say he had seen it tried. Not choosing to sacrifice a pair of gloves to the trial of an effect I had no belief in, I wrapped a newspaper double about my hand, and plunged it in up to the wrist. I retained my hand in the tar longer than I could when naked without feeling any pain.

I am, dear Sir, very truly yours,

Twickenham, Nov. 1, 1816. RICHARD DAVENPORT.

P.S. As the sense of feeling must differ much in different persons, I have since tried to compare the sensation produced by hot water with that which I have described above; and I can say decidedly that I cannot bear the heat of water at 140° so long as that of tar at 220°.

ARTICLE VI.

Some Observations respecting the Geology of South Wales.

By W. H. Gilby, M.D.

THE short, though excellent, paper of Mr. Martin, in the Philosophical Transactions for 1806, contains most of the particulars of any interest regarding the South Wales coal tract, and forms a striking contrast with the long and wearisome narration with which the pages of some of our publications are overspread. The following facts, however, which occurred to me in a late tour through South Wales, shortened and continually interrupted by heavy rains, appear to me new, and in some measure interesting:—

1. Mr. Martin states, and so does Mr. Townsend, that the coal deposite of South Wales rests every where upon the mountain lime-stone, the course of which will be better understood by referring to the map annexed to Mr. Martin's paper. Now the term mountain lime-stone has always been applied to a rock varying a good deal in colour, but pretty uniform as to its general character, and composed chiefly of carbonate of lime, with some properties of argillaceous matter. But in two places I found a rock immediately subjacent to the coal strata, and heretofore considered as part of the common mountain lime-stone chain, which is a variety of magnesian lime-

stone containing 38 per cent. of carbonate of magnesia. One of the places to which I allude is the high ridge of ground which runs east and west to the south of Caerphylly, about four miles north of Cardiff. I walked for a considerable distance along this ridge, and still found no appearance of the true mountain lime-stone; and as the range appeared to run for many miles east and west in a continuous line, it is probable that the magnesian lime-stone will be found to occupy a considerable part of its course. The next place to which I allude is the hill which overhangs Risca in coming from Machen, the greater part of which is composed of the same variety of magnesian lime-stone, and is there most distinctly immediately subjacent to the coal measures which make their appearance, I believe, even in the village of Risca. This variety of magnesian lime-stone is of a grey colour, hardly different from that of the usual varieties of mountain lime-stone; but, independent of chemical criteria, it is readily distinguished from that rock by its greater specific gravity, by its greater hardness, and by its possessing a highly glimmering lustre: whereas the mountain lime-stone, when free from scales of calc spar, is generally quite dull. The observations of Mr. Tennant had established the opinion that magnesian lime is injurious to land, when used in considerable quantity; but we learn from Dr. Thomson, that this kind of lime is used as a manure in the North of England, with very little caution, and that it is productive of no bad consequences. The same is the fact in the districts to which I have alluded. I found the hill at Risca and the ridge south of Caerphylly crowned with quarries and lime-kilns, and was informed that the lime is very extensively employed for agricultural purposes, without any idea of its having properties different from those of common lime. A quarryman however was aware that this stone took a longer time to burn than common lime-stone, as for instance the lyas.

It is probable, when this species of magnesian lime-stone shall have become more generally known, that it will be found frequently associated with the mountain lime-stone; but in colour it so much resembles that rock, that few would at first sight be led to suspect any thing remarkable in its chemical composition. I discovered some time ago *precisely* the same variety of magnesian lime-stone in the neighbourhood of Bristol interstratified with the mountain lime-stone, and have lately transmitted an account of it, together with its analysis, to the Geological Society. I have, besides, met with a more crystalline species lying upon the mountain lime-stone near Ross; and Mr. Wm. Clayfield showed me a specimen of the *mountain magnesian lime-stone* collected by Mr. Tennant, I believe from the Mendip Hills.

2. In former communications I had hazarded the conjecture that the old red sand-stone will generally be found under the first floetz or mountain lime-stone, and wherever I met with the latter rock in South Wales this was always the case. At Risca, and in the range below Caerphylly, the magnesian lime-stone, which is the

locum tenens of the mountain lime-stone, lies upon the old red sand-stone and its accompanying conglomerate. In walking from Myrthyr Tydvil to Brecon, the mountain lime-stone makes its appearance for the first time about three miles from Myrthyr, and forms on each side of the road a cliff of considerable height. About a mile and a half further it ceases; and then succeeds the red sandstone conglomerate; and shortly afterwards the old red sand-stone in its true character. It is seen on each side of the road, as far as the little river Grue. A mile or two onwards I found myself at the foot of a high mountain, called the Van or Brecon Beacon, which I ascended, and descended on the opposite side, and found that the whole of it, from top to bottom, is composed of the old red sandstone, in strata dipping S.S.W. under the mountain lime-stone. The summit of this mountain commands a very extensive view. To the west are seen other lofty hills, presenting the same kind of contour as the Van, which are, therefore, probably composed of the same kind of rock. The country to the north of the Van presents a hilly appearance; but the height of the hills seems very inconsiderable when compared with that of the Van and of those on the west. In fact, the configuration of the country at once shows an alteration in its geological structure.

3. The coal measures on South Wales are almost identically the same with those in Gloucestershire and Somersetshire. They are micaceous sand-stone, clay-stone, slate-clay, quartzy sand-stone, besides which they have many beds of clay-iron-stone; and I have also seen a curious kind of conglomerate very like that which occurs in the Dudley coal-field.

4. A little beyond Llanvihangel between Brecon and Builth, I took the old road over a hill, where the geology of the country assumes a new character, as the hill is almost entirely formed of clay-slate. Some of the strata abound with impressions of shells, and of a very singular minute body, which I had never seen before, resembling somewhat a limpet. The dip of the strata in this hill is contrariwise to that of the Van, and its escarpement, together with that of the adjoining eminences, is turned towards the S.E., whereas the grand and precipitous façades of the Van are opposed to the N.E. These circumstances are highly interesting to the geologist, as it is clear that they could only have arisen from some remarkable difference in the formations themselves, as well as in the periods at which they were deposited; and they become doubly interesting when we find them in accordance with a system which we have been accustomed to follow, of the truth of which they may then be taken as a kind of *natural evidence*. Thus we have seen that the rocks to the south of Brecon are composed of the earliest members of the floetz formation, while those to the north of it consist of clay-slate containing organic remains, which is more to the north (as hereafter to be shown) associated with graywacke, and they are, therefore, to be considered as belonging to the transition class. In the Island of Arran, near Loch Ranza, the difference

between the floetz and the older rocks is exhibited in a similar way. The strata of the old red sand-stone, and those of the clay-slate, dip in opposite directions, and consequently their outgoings abut against each other. Mr. Jameson has given a faithful drawing of this appearance in his Tour through the Western Islands.

5. No other rock is seen on the road to Builth excepting clay-slate, which is constantly varying in its dip, but the hills generally present a precipitous front to the south.

6. The hills near Builth, after passing over the bridge on the Wye, are highly curious; but incessant rain for four days hardly allowed me an hour's walking. About half a mile from the town on the road to Presteign, there is a small chapel at the foot of a hill. In this hill I found several varieties of rock, viz. of clay-slate, of a kind of clay-slate porphyry, and an amygdaloid. In the space of a few yards the strata seemed to dip to every point in the compass; and in some places were considerably inclined, and at others vertical.

7. A collier had lately found interstratified with the clay-slate a lime-stone full of shells, and sometimes containing the remains of the encrinus. The beds are very thin; otherwise in that country the discovery would be a very valuable one. From the representations of this man, some adventurers were trying for coal; and, from every thing I saw of the country, without any prospect of success. It is in such circumstances that a knowledge of mineralogy and geology is so eminently useful, as the mere practical miner may be deceived by a thousand illusory appearances. The collier, for instance, and the inhabitants, showed me a stone of a black colour, upon which they seemed to place great reliance as an indication of coal. The stone, however, appeared to me the common clay-slate impregnated with bituminous matter, being in fact a variety of black chalk or drawing slate.

8. Between Builth and Rhyader I observed little else than clay-slate, and a very hard kind of sand-stone, till I came within three miles of Rhyader, when I found myself under a very high and beautiful cliff of greywacke* in strata, dipping to the north. This rock was associated with the transition sand-stone just now spoken of. To the greywacke clay-slate again succeeded, which I think is the prevailing rock round Rhyader.

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* By greywacke, I understand a rock answering in description to Mr. Jameson's definition of it, as consisting of a basis of clay-slate, containing fragments of clay-slate, portions of quartz, and scales of mica.

Article VII.

Notes of a Mineralogical Excursion to the Giant's Causeway.
By the Rev. Dr. Grierson.

(Read before the Wernerian Society, March 30, 1816.)

After having in the month of September last made the few observations on the Loch Doon granite, of which I had the honour to communicate some account to the Society a few weeks ago, I set out on a short excursion to the North of Ireland. Having little more than a fortnight to spare for the accomplishment of this journey, the observations I had it in my power to make could be neither numerous nor very important; yet I did observe some things which I hope may not prove altogether uninteresting, and shall, therefore, beg leave to state them as shortly as I can. On the 11th of the month I left the neighbourhood of Balmachellan Kirk, about two miles to the N. E. of New Galloway, and proceeded along the eastern bank of the Lake of Ken to the point where that lake is joined by the River Dee (six miles); no rock but greywacke and its slate hitherto appearing, though the Dee district of granite skirts the lake for about a mile on the opposite bank. I crossed this lake at the Rhone Ferry, and landed on the south side of the Dee. The picturesque variety and richness of the scenery at the junction of the river and lake, where stands the beautiful seat of Airds of Kells, are greatly admired by strangers. In my opinion they are not easily paralleled; but here I may be justly suspected of partiality, it being my native place. From hence I proceeded southward, ten miles, to Galehouse of Fleet, and saw no other rock but the transition one, of which indeed almost the whole of the two counties of Kirkcudbright and Wigton are composed, except the three well-known districts of granite. When I had passed the Lake of Lochenbrech, near which is the celebrated chalybeate spring, and was now approaching the granite mountain of Cairnsmuir, I began to perceive, to the east of it, and at the distance of at least three miles from the hill, vast numbers of rolled pieces of granite, some of them of great size, scattered over the transition country. The country about Gatehouse is all of this sort (transition), and there is no other rock to be seen the whole way from thence to the harbour of Portpatrick (49 miles), where the cliffs of greywacke appear in great magnificence. The greywacke and slaty strata of this track are all in the usual direction, and in general highly inclined. The view around Galehouse, situated at the mouth of the river Fleet, and beside the magnificent mansion of Cally, is uncommonly fine; and the drive from thence along the high and wooded shore of the Bay of Wigton, in sight of Sanbees Head and the Isle of Man, to some miles beyond Creetoun, is esteemed one of the most beautiful and interesting in the kingdom. On the right towers the lofty granite mountain of Cairnsmuir, but no part of the granite reaches the high road. In the greywacke formation, a few miles to the east of Newton Stewart, and only three or four miles from the granite of Cairnsmuir, were formerly worked pretty extensive lead-mines. They have now, however, for some years been abandoned. It is the opinion of the best mineralogists that the galena veins found here are a continuation of those so extensively wrought at Leadhills and Wanlockhead. They are, I believe, found to run in the same direction, and they are in the same kind of rock. The county of Wigton presents altogether a flat uninteresting appearance, and is, so far as I know, all transition. As I already hinted, the transition rocks appear in high cliffs and rugged beauty at the harbour of Port Patrick; and on arriving in Ireland, I found them to accompany me for about eight miles on the road from Donaghadee to Belfast. The county then becomes trap. The rock appeared to me to be a variety of porous ill-formed clink-stone or tuff, loose in its texture, and of a greyish or iron-black colour.

At the town of Belfast this formation still continues, where the flourishing city of 30,000 inhabitants, the river, the bridge of 21 arches, the bay, the shipping, the rich county of Down extending almost as far as the eye can reach to the east, and the perhaps still richer county of Antrim stretching along the north-west shore of the bay by Carrickfergus on the north, the Black and Cave mountains rising to the height of Arthur's Seat or so, to the west, form a picture which, to my imagination at least, is not easily surpassed. The Black and Cave hills to the west of Belfast are composed of green-stone, basalt, or tuff. In these trap rocks there is an immense bed of greyish-white indurated chalk, or conchoidal lime-stone. Above this bed, which is worked in various parts, reposes some hundred feet of the trap; and in the lime-stone or chalk are numerous and large nodules of grey flint. Some of the flint nodules are of a cochineal-red, extremely beautiful. Between the lime-stone and the green-stone, and in so far as I had the opportunity of observing on the lower side of the former, is a rock commonly called mulatto-stone, of a light grey speckled colour, and which seems to be lime-stone mixed with particles of the trap. On this trap, at the bottom of the hill, between it and the town, rests a vast bed of clay; and in the bed, where it is intersected by a rivulet to a considerable depth, I had the opportunity of observing and picking up some fine specimens, from numerous veins, of foliated and fibrous gypsum. These veins are seen to run principally, I think, in a northerly and southerly direction, and to dip towards the east. I observed them from an inch to a foot or more thick.

Leaving Belfast, I pursued my way to the Giant's Causeway, by Antrim, Ballymoney, and Coleraine (60 English miles). The formation all along this track is the newest floetz trap. I could observe no other rock but a sort of loose textured greyish-black one, appearing to be basalt or tuff, except here and there some green-stone, and a reddish-white stone, having much the same appearance, hardness, and specific gravity, as the indurated chalk, with a conchoidal fracture, imbedded in the trap near Temple Patrick, about eight miles north-west of Belfast.

Near Coleraine, on the river Bann, which issues from the magnificent Lough Neagh, and meets the North Sea a little way below the above-mentioned town, 12 miles west of the Giant's Causeway, at a fall called the Cuts, is a formation of green-stone. I made inquiries with respect to the siliceous petrifactions said to be formed by the waters of Lough Neagh, and found that this phenomenon was universally believed; but I could not obtain a specimen. The country from Belfast to Coleraine is, in a general view, flat, but it is by no means what is called a dead flat. Wavings to a great extent strike the eye; and to the east, at a distance, rise large conical hills, such as usually characterise a trap district. To the west, in the distance, towers the high mountain of Innishoue, which is probably primitive. At Antrim, on the east bank of Lough Neagh, where stands also Sheans Castle, the seat of Lord O'Niel, the country appeared to me eminently rich and beautiful, as indeed the whole route from Belfast to Coleraine, with the exception of a few miles between Ballymena and Ballymoney, is rich and fine. Here and there, however, notwithstanding this, we have extensive bogs, or peat mosses, as they are called in Scotland.

I left Coleraine on the morning of Sept. 17, in company with a gentleman of that place, whose obligingness, intelligence, hospitality, and kindness, afforded me a most agreeable specimen of the Irish character, and proceeded to the Giant's Causeway. The day was charming; and it is not easy for me to express the gratification I felt, as we made our way through a fine and gently varied district, at the idea of having it in my power soon to contemplate in favourable circumstances one of the most stupendous and interesting natural phenomena that are any where to be seen. From Coleraine to the Causeway is eight miles in a northerly direction, and I could observe no rock on our way but the trap formation. On crossing the river Bush at the village called Bushmills, the country begins gradually to rise, and we descry about two miles before us a ridge of considerable height, seeming to terminate quite abruptly on the other side. What we perceive is the land side of the precipice of the Giant's Causeway. It seems to have been a hill of basalt, with nearly perpendicular columnar concretions, cut in two, as it were, by a vertical section, and the half of the hill next the sea carried away. On getting in front of this precipice, which you do by a pass on the west side of it, a most stupendous scene presents itself. The precipice, extending for a mile or two along the shore, is in many places quite perpendicular, and often 350 and 400 feet high, consisting of pure columnar basalt, some of the columns 50 feet in perpendicular height, straight and smooth as if polished with a chisel. In other parts the columns are smaller, inclined, or bent; and a less length of them strikes the eye. From the bottom of this precipice issues, with a gentle slope of about 1 in 30 towards the sea, an immense and surprising pavement, as it were, consisting of the upper ends of the fragments of vertical columns of basalt that have been left when the seaward half of the basaltic hill was carried off. The ends of these columns are in general 15 or 20 inches in diameter, some of them of three sides, some four, five, six, seven, eight, or even nine. Five and six sides seem to prevail most. From the bottom of the precipice to the sea at low water along this pavement or causeway, which, from the artificial appearance it puts on, has, doubtless, in a rude age, given name to the place, is a length of 730 feet. It has been observed to proceed into the Ocean as far as can be traced by the eye in a calm and clear day. To any person who has seen both this place and Staffa, the idea naturally enough suggests itself that they are parts of the same once continuous immense bed of columnar basalt. There are properly three pavements proceeding into the sea, distinguished by the names of the Great Causeway, the Middle Causeway, and the West Causeway. These are three large gently sloping ridges of the ends of basaltic columns, with depressions between them, covered with large blocks or masses, that seem to have from time to time been detached, and rolled from the precipice. I had no opportunity of perceiving with what rocks the basalt of the Giant's Causeway is connected. I am told conchoidal white lime-stone meets it on both the east and west sides. There is in one place near the east side of the Great Causeway a green-stone vein eight or ten feet wide intersecting the basalt from north-west to south-east.

There was now pointed out to us by the guides a singular enough and curious phenomenon, and which is particularly interesting, as it has been thought by those who hold the igneous origin of basalt to be a confirmation of their doctrine. Nearly opposite to the West Causeway, and within about 80 feet of the top of the cliff, is found to exist a quantity of slags and ashes, unquestionably the production of fire. On ascending to this spot, which can be easily done, I found the slags and ashes deposited in a sort of lead about four feet thick, and running horizontally along the face of the basaltic precipice 20 or 30 feet. The ashes are in general observed to lie undermost, and the slags above them. They are covered with a considerable quantity of earth and stones, which all consist of basalt, are of a large size, some of them three or four feet or more in diameter, and the ashes likewise rest on the same sort of materials. What struck me here was, that these ashes and slags are entirely unconnected with any rock or formation which seems to be *in situ*, or in its original position. They are, therefore, in my opinion, distinctly artificial, and nothing more than the remains of some large and powerful fire which had been kept burning for a long while on the top of this precipice, either for the purpose of a signal, or some other which we cannot now ascertain; and that, owing to the part of the cliff on which the ashes were lying having given way and tumbled down, they have been thus buried beneath the ruins and there remain. This hypothesis may appear to some fanciful or extravagant; but I should have little hesitation in referring the truth of it to any unprejudiced person accustomed to investigations of this sort who will be at the trouble to scramble up and survey the spot. Nay, I think I could even trust the decision to a Huttonian himself! The mass of materials in which the slags and ashes are found is clearly moved from its place, and has distinctly the appearance of a large *slip* of loose pieces of rock and soil that has been disengaged by means of frost or some other agent. It may have been effected by an earthquake; or the fire itself may have contributed to its own removal by the rents or cracks its heat made in the rock on which it stood. It is not a great many years since these ashes were noticed. John Corry, one of the most obliging and intelligent guides about the place, picked up some of them on the beach below, and naturally enough concluding that they came from the cliff above, he climbed up and found their repository. One gentleman, he informed us, who is well known to have paid much attention to the appearances at the Giant's Causeway, and who has written upon the subject, will not yet believe that the ashes are found in the place which I have described, but insists (obstinately enough, no doubt!) that honest John and his colleagues have put the ashes there on purpose to deceive the public! He cannot be prevailed upon to scramble up and look at the ashes himself, verifying, it would seem, the old proverb, which says, that there is no one blinder than he who will not see.

A considerable way from the repository of the ashes and slags, and to the east of the Great Causeway, is another curious appearance. Here, in the pure basalt, 70 or 80 feet from the top of the cliff, is a horizontal bed of wood coal eight feet thick. The coal to all appearance rests immediately on the basalt below, and the ends of perpendicular basaltic columns are seen distinctly to rest on it above. The basalt is not in the least changed by the contact of the coal, nor the coal by that of the basalt. The coal is very beautiful and distinct, and in one place is seen a *coalified* tree, if I may use the word, 10 or 12 inches in diameter running directly in below the basalt.

Within sight of this spot, and about 300 yards to the east of it, are the beautifully conspicuous basaltic pillars, 45 feet long, and vertical, with the longest ones in the middle, and the others gradually shortening towards each side like the columns of an organ. From this appearance they have received the appropriate name of *the organ*. At the bottom of this cliff, by examining and breaking the loose columnar pieces of the rock that have fallen down, we found many fine specimens of calcedony, zeolite, and semi-opal. These occur in cavities in the basalt. Sometimes the cavity is not completely filled with the calcedony or opal; and when that is the case, the empty space is observed to be always the upper part of the cavity while the rock is *in situ*. Moreover, the surface of the calcedony or opal next to the empty space is always found to be flat and horizontal, which would show that the substance must have been filtered into its situation in a fluid state, and afterwards consolidated.

When I left Belfast for the Giant's Causeway, it was my intention to have proceeded on to Ballycastle and Fairhead, where I understand is an ample field of most interesting investigation for the mineralogist. Dr. M'Donnell, of Belfast, informs me that on the east side of the latter place we have coal and basalt within a quarter of a mile of a primitive rock mica slate. From the above gentleman, to whom I had the good fortune to be introduced, I received the most polite and kind attention, and very ample information with respect to the geognostic appearances to be observed at the Giant's Causeway, and round the coast road. But as my time was limited, I was under the necessity of reluctantly abandoning my intention of visiting Fairhead, and of returning to Belfast by the way in which I had come. Dr. M'Donnell has a large and interesting collection, well worth seeing.

On returning to Gatehouse of Fleet, and being called by business to the shore of the Solway Firth at Airds and Balcarry, situated on a portion of the Galloway transition country, nine miles to the south-west of Kirkcudbright, I took the opportunity of examining a few miles of the bold rocky coast there. It is sufficiently interesting. Here within a mile of one another we have the granite, the transition rocks, and the old red sand-stone. I observed the granite of the Criffle district within a mile of Balcarry House seemingly very near the greywacke, but could nowhere see a junction. On the sea shore in the Creek half a mile to the south-east of Balcarry House, opposite the Isle of Heston, is a granite vein of about 10 inches thick in a rock quite similar to the compact or fine-grained gneiss of the Lauran. This vein cannot be traced to the granite rock. On walking about a mile further round this shore to the westward, in company with Mr. Brown, Land Surveyor, along tremendously high cliffs of greywacke and greywacke-slate (the strata very highly inclined, and in the usual direction), we came upon an interesting and very distinct junction of the above rocks with the old red sand-stone. This junction is completely exposed by the sea, and affords a fine opportunity of observing on a large scale the influence of the one formation on the other. The transition strata (slate) are elevated about 70°, and dip towards the sand-stone, the strata of which are conformable in position. For 15 or 20 yards you can see the one rock affecting the other; that is to say, at about the above distance from the pure slate, the sand-stone becomes evidently penetrated by the slaty matter, and continues more and more so as we advance, until it ends in pure slate. Half way between them is a substance which is neither the one nor the other, but a *tertium quid* formed of the two. In short, the passage of the transition and floetz rocks into one another is here very striking.

About a mile and a half further west along this shore, near Rascaril, we found in the old red sand-stone a bed of sandy lime-stone 40 feet thick, and traceable for nearly twice that distance, and there is in this formation, half a mile from the junction I before described, evident indications of coal. It crops out in different places, but the trials for working it that have been hitherto made have not been attended with success.

On the road from Balcarry to Castle Douglas, near Orchardton, we fall in with the granite for a short way, but soon again meet the transition country, which extends all along the valley of the Ken.

Article VIII.

Chemical Classification of Minerals. By Mr. Sowerby, F.L.S.

(To Dr. Thomson.)

Sir,

The recent appearance of an introductory publication on mineralogy, in which the minerals are arranged in an order dependant on their contents, induces me to forward to you for publication a sketch of a system * adopted many years ago, because it does not appear to be generally known. I would recommend it in preference to other artificial systems, because it contains a fixed rule for the arrangement of every substance when its nature is known; and it is capable of modification with facility as fast as the science of chemistry advances. Some changes have been lately made, and others may be introduced; but I prefer laying it before the public in its present form, rather than have to make frequent alterations, which might be the case were I to adopt too suddenly all the changes chemistry has of late undergone. I shall be happy to receive suggestions of any kind by which it may be improved, as also any addition to the list of minerals accompanying the sketch.

As soon as, from various considerations, I had determined to arrange minerals from their analysis, I sought for some property or character, which, like the fructification of plants, should be possessed by every substance that enters into their composition, and according to the modifications of which such substance might be disposed, as a basis for the arrangement of minerals. The specific gravity of the elements fully answered my wishes, whereupon I divided these elements into three classes: 1. Combustibles: 2. Earths: 3. Metals, placing the lightest class first, then the lightest individual at the head of the class, and proceed to the heaviest, calling each a genus: every compound was next placed under that genus to which belonged either what chemists termed its base, or else that substance of which it contained the greatest quantity, placing at the head of the genus that mineral, if such occurred, which was uncombined; and having arranged the substances in each of the compounds according to their quantity, placing that mineral next which contained the first ingredient according to the order of the genera in the manner words are arranged in a dictionary, calling each different combination a species. For the more easy arrangement of all the combinations a chemist is capable of producing, I found it convenient to divide the classes into orders distinguished by the substances included under them, being either binary combinations, as in the first order of combustibles, or combinations of binaries with each other, as in the second order of combustibles, in which many of the species of the first became genera of the second. Thus ammonia would be a species of the genus nitrogen in the first order; and sulphate of ammonia, a species of the genus ammonia in the second order; but this plan I have found too complex, inconvenient, and indeed unnecessary among minerals; so I now throw aside the orders, or rather blend them together, and arrange sulphate of ammonia as it would stand in the second order had the orders been retained. The same method is followed with the other alkalies, so that they come agreeably together. I have done the same with the other combustibles and with the metals: but in the class Earths I make two orders; the first *homogeneous*, containing simple minerals; the second *aggregate*, for the rocks. In conformity with this plan, the following list of all the species of minerals, as far as I can make them out, is drawn up. I would recommend the division of such species as carbonate of lime when there is little or no chemical difference among varieties of very different external characters, into sections, so that arragonite will be a section of the species carbonate of lime; every species also that occurs in as many forms should be divided into, 1. Crystallized: 2. Of particular shapes: and, 3. Amorphous, as has been in some measure done by Babington.

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* Published in my Catalogue of British Minerals in 1811, and in my British Mineralogy, with figures, periodically.

This system is perfectly artificial. I should be glad to see a natural one, founded upon some universal and constant characters of easy acquirement; but as it is always necessary for the discrimination of minerals to know their contents; and as their more accessible characters only serve as indications of them, and are generally comparative, it is to be feared no better foundation for a system will be discovered.

List of Minerals, arranged in Classes, Genera, and Species, by Mr. Sowerby, with References to Figures identifying them in British and Exotic Mineralogy.

The former of these works is just completed, with figures of British minerals; and I hope will be found greatly to assist the study of mineralogy, since figures are always more intelligible than the most laboured descriptions. Indeed, descriptions are so generally couched in technical terms, that it is necessary to learn almost a language to comprehend them.

COMBUSTIBLES.

Genus Oxygen.

Sp. Ice.

Ammonia.

Muriate,
Sulphate.

Potassium.

Nitrate.

Sodium.

Muriate,
Sulphate,
Glauberite,
Carbonate,
Borate,
Criolite.

Sulphur.

Native,
Sulphuric acid.

Carbon.

Diamond,
Stone coal,[*]
Bituminous coal,
Jet,
Bitumen,
Dysodile,
Retinasphaltum,[†]
Wood coal,
Peat,
Amber,
Plumbago.

Boracic Acid.

Native.

Appendix. Improper Minerals.

Common air,
Carbureted hydrogen,
Mineral tallow,[‡]
Carbonic acid.

EARTHS.

Argilla.

Sp. Corundum,
Hydrargyllite,
Subsulphate of
Alum,
Mellite,
Topaz,
  § 1. Pyrophysalite,
  § 2. Pycnite,
Spinelle,
  § 1. Pleonaste,
  § 2. Automolite,
Cymophane,
Blue Feldspar,
Lazulite,
Fibrolite,
Cyanite,
Andalusite,
Sommite,
Pseudo-sommite,
Pinite,
Granatite.

Magnesia.

Native,
Hydrate,
Sulphate,
Carbonate,
Borate,
Serpentine,
Chrysolite ? (See silex.)[§]

---

[*] Stone coal: this is in many cases the remains of bituminous coal deprived of its bitumen.

[†] Retinasphaltum: the Highgate resin, or fossil copal, belongs to this species. (See Gentleman's Magazine, vol. xxxii. p. 108.

[‡] Mineral tallow is only the remains of some animal that has been buried in a peat bog.

[§] Chrysolite: some of the analyses give most silex, making the genus doubtful.

Lime.

Native,
Nitrate,
Subphosphate,
Phosphate,
Sulphate,
  § 1. Anhydrous,
  § 2. Gypsum,
Carbonate,
  § 1. Arragonite,
  § 2. Common carbonate,
Magnesian carbonate,
Dioptase,
Fluor,
Datholite,
Arseniate of lime,
Tungstate of lime.

Silex.

Quartz,
  (Silex and water,)
Opal,
  (Silex and alumina,)
Calcedony,
  § 1. Calcedony,
  § 2. Jasper,[*]
Iolite,
  (Silex, alumina, and water,)
Bildstein,
Rocksoap,
  (Silex, alumina, and potash,)
Feldspar,
Triphane,
Lepidolite,
Mica,
Leucite,
  (Silex, argilla, potash, and soda,)
Pitch-stone,
Gabronite,
Fat-stone,
  (Silex, argilla, and soda.)
Sodalite,
Analcime,
Chabasic,
Dipyre,
Hyalite,
Wernerite,[†]
Prehnite,
Mesotype,
Stilbite,
Laumonite,
Aplome,
Hauyne,
  (Silex, argilla, and glucine,)
Emerald and beryl.
Euclase,
  (Silex, argilla, and barytes,)
Staurolite,
  (Silex, argilla, and oxide of iron,)
Anthophyllite,
Axinite,
Tourmaline,
Garnet,
Epidote,
Hornblende or actinolite,
Actinolite ? [‡]
  (Silex and magnesia,)
Talc,
Asbestus,
Bronzite ? [§]
Olivine,
Jade ?
  (Silex, lime, &c.)
Schaalstein,

---

[*] Jasper: the iron that is often abundant in this is not apparently sufficiently constant to make a species.

[†] Wernerite: this includes scapolite. One or two per cent. of alcali does not seem sufficient to separate them. In general, any quantity of an ingredient less than five per cent. is deemed accidental; but in a few instances, as with respect to the alkalies, that quantity seems characteristic, and is noticed.

[‡] Actinolite of Bournon analysis not known; but its characters are so like those of hornblende, that its analysis probably would not differ materially.

[§] Bronzite: query if the magnesia in this arises from the serpentine that includes it, or is it distinct from smaragdite.

Ichthyophthalmite,
  (Silex, lime, alumine, &c.)
Smaragdite,
Idocrase,
Kaneelstein,
  (Silex, lime, and magnesia.)
Tremolite,
Augite,
  (Silex, iron, &c.)
Hyperstein,
Jenite,
  (Unknown,)
Bergmannite,
Fahlunite,
Humite,
Indianite,
Lapis Lazuli,
Macle,
Meionite,
Mellilite,
Spinellane,
Spinthere,
Turquois.

Strontia.

Sulphate of,
Carbonate of.

Barytes.

Sulphate of,
Carbonate of.

Zirconia.

Hyacinth.

Yttria.

Gadolinite.

EARTHS (*aggregate*).

Argill.

Rotten-stone,
Cimolite,
Meerschaume,
Macle,
  § 2. Indurated,
Clay,
Shale,
  § 2. Bituminous,
Loam,
Ochre,
Alum clay.

Lime.

Flexible lime-stone,
Sandy lime-stone,
  § 2. Talciferous,
  § 3. Toad-stone.

Quartz.

Breccia,
Grit,
  § 2. Calcareous,
  § 3. Micaceous,
  § 4. Ferriferous,
Hone-stone,
Greywacke,
  § 2. Schistose,
Fullers-earth,
Granite,
  § 2. Gneiss,
Sienite.

Feldspar.

Granitic,
  § 2. Schistose,
Porphyry,
  § 2. Decomposing.

Mica.

Granitic,
  § 2. Schistose,
  § 2. Schist.

Pitch-stone.

Porphyry.

Hornblende.

Granitic,
  § 2. Schistose,
Basaltes,
  § 2. Cellular.

Talc.

Talcose slate,
Lithomarga.

Debris.

Lava,
Schistose debris,
Lime-stone debris,
Vegetable debris.

METALS.

Tellurium.

Native,
Plumbiferous,
Auro-plumbiferous,
Argento-auriferous.

Antimony.

Native,
  § 2. Arsenical,
Protoxide,
Peroxide,
Sulphuret,
  § 2. Supersulphuret,*

Manganese.

Silical protoxide,
Oxide,
Ferriferous phosphate,
Sulphuret,
Carbonate.

Zinc.

Protoxide,
Peroxide,
Silical oxide,
Sulphuret,
Sulphate,
Carbonate,
  § 2. Hydro-carbonate,

Tin.

Oxide,
Sulphuret.

Molybdenum.

Oxide,
Sulphuret.

Cobalt.

Oxide of,
Sulphate,
Arseniate.

Iron.

Native,
Nickeline,
Protoxide,
Oligistic,
Peroxide,
Hydroxide,
  § 2. Pitchlike,
Phosphate,
Subsulphuret,
Sulphuret,
Prismatic sulphuret,
Subsulphate,
Sulphate,
  § 2. Cupreous sulphate,
Carbonate,
  § 2. Argillaceous carbonate,
Arseniate,
Scheelate,
Chromate.

Nickel.

Native,
Kupfer-nickel,
Oxide.

Arsenic.

Native,
Oxide,
Sulphuret,
  § 1. Yellow,
  § 2. Red,
Cobaltiferous,
  § 2. Grey cobaltiferous,
Ferriferous,
  § 1. Argentiferous.

Uranium.

Oxide of,
Peroxide,
  (Hydrate, when green.)

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* Sulphuret of antimony: Bournon finds the crystals of red antimony to be the same as those of the grey. I conceive the red to be a supersulphuret free from oxygen.

Copper.

Native,
Protoxide,
Peroxide,
Hydrate,
Silical hydrate,
Muriate,
Phosphate,
Subsulphuret,
Sulphuret,
Granular sulphuret,
Buntkupfererz,
Antimonial sulphuret,
Grey sulphuret,
Arsenical sulphuret,
Sulphate,
  § 2. Ferriferous,
Green carbonate,
Blue carbonate,
Arseniate,
  § 1. Obtuse octohedral,
  § 2. Hexahedral plates,
  § 3. Tetrahedral prisms, fibrous globules,
  § 4. Triedral prisms,
Ferriferous arseniate.

Bismuth.

Native,
Oxide?
Sulphuret,
  § 2. Cupreous,
  § 3. Plumbo-cupreous,
Carbonate.

Silver.

Native,
Muriate,
Subsulphuret,
  § 2. Antimonial,
  § 3. Cupreous,
Flexible subsulphuret,
Red sulphuret,
Brittle sulphuret,
Black silver,
Carbonate of,
Antimonial.

Lead.

Native,
Oxide of,
Phosphate,
Sulphuret,
Endellium,
Sulphate,
  § 2. Sulphureted sulphate,
Carbonate,
Rhomboidal carbonate,
Prismatic carbonate,
Murio-carbonate,
Molybdate,
Arsenite,
Arseniate,
Chromate.

Palladium.

Native.

Quicksilver.

Native,
Muriate,
Sulphuret,
Argentiferous.

Gold.

Native,
  § 2. Argentiferous.

Platinum.

Native,
  § 2. Ferriferous.

Titanium.

Ruthile,
Anatase,
Sphene,
Menachanite.

Chrome.

Oxide.

Tantalum.

Tantalite,
Yttro-tantalite.

Iridium.

Osmial.

Cerium.

Cerite,
Allanite.

Unknown.

Crichtonite.*

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* Crightonite is not proved to be a metallic ore.

ARTICLE IX.

The Conclusion of the Essay on the Explosions in Coal-Mines.
By Mr. John B. Longmire.

(To Dr. Thomson.)

DEAR SIR, *Nov.* 9, 1816.

In my former communication on the explosion of coal-mines, which you did me the favour to insert in the *Annals of Philosophy* for November, I described the properties of the air's explosive motion: in this paper I will give an example of its effects on the mine.

Let A B C D, [Plate LVIII. Fig. A] (see the number for November) be part of a coal-mine, in which A is the pit shaft; A B, A D, *b c*, *a* C, *e d*, are ranges, that contain doors and stops in all the thirls, or cross workings. A door is represented by two lines, as at *k*, and a stop by one line, as at *f*. By means of these doors and stops, the current of air, which descends from the surface down the shaft A, passes round the mine, as the dotted line A, B, *c*, C, *n*, *i*, *m*, *q*, *l*, *g*, represents: at *g* it commences to circulate round other workings till it reaches the up-air pit.

Let inflammable air enter the mine at the forehead, *h*, of the working *h i*. As the current of air does not traverse this working, it is filled with inflammable air from *h* to *i*; and the current of air moving along the working *i* D carries the inflammable air with it as it enters this working. But if the door *e* be left open, the air passes to *g* by a route, *e* 2 *g*, shorter than its usual way. Hence the gas accumulates in the working D *i* at *i*; and if a miner travel along this working, without detecting the change in the current of air, he sets the gas on fire, and an explosion is the consequence.

The explosion flies with an equal velocity, in the directions *i* A, *i k*, and *i* D. It is soon stopped by the doors in the direction *i* A. It proceeds to D, gives small concussions to the air in the thirls, *n*, *b*, &c. as it passes them; and after it has reached the forehead, it returns up the working with a decreasing velocity. This rebound the workmen call *a return*. If all the stops in the thirls of the range *n* D withstand the shocks given to the air in them, by the advancing and retreating motion, this part of the explosion soon breaks through the thirl *m*, and there meets another part coming down the working *k t*. The air in this part of the explosion passes along the inclined thirl *i k*. It there blows down the door *k*; which for a moment retards its progress, and produces a small rebound. But the door being overturned, the explosion strikes the side at *k*, flies to the opposite side at *n*, returns to *e*, then passes alternately from *e* to *b*, and from *b* to *p*; from *p* a part of it enters the working *p q*, and the other part flies to *r*, and displaces the door *r*; from that place it passes to the opposite side *s*, and then strikes the forehead at *t*, and rebounds up the working from side to side.

The other part of the explosion, which continues to the side *p* of the working *p q*, flies from *p* to *u*, and from *u* to *v*; from *v* it passes to *w*, in the open thirl *w x*, and then from *w* to *x*; from *x* it strikes the far side of the working *y g* at 3; it then moves to *y*, and from *y* it reaches the corner 2 of the pillar 5 *d*, one half being reflected to 1, and the other to 4. From 1 it flies to 2, from 2 it strikes obliquely the corner of the forehead *q*, and from thence retreats backwards along the thirl 2, 1, and down the working *q p*. The other part of the explosion passes through the strait thirl, from 2 to 4, and from 4 to 5; at 5 it displaces the stop, and flies to 6 on the opposite side of the working *d e*; from 6 it passes to 7, and from 7 to 8. In this irregular course it proceeds through the workings in the part C E *e*; and gives successive shocks to the air in every one of them. When the explosive motion has pervaded all the workings in the part E C, it can only escape through the narrow working *g*, and the stone tunnel at B. But the displaced air cannot pass through these outlets as fast as the heat dilates it, without having first obtained an augmentation of pressure in every part of the workings. In acquiring a greater degree of pressure, a current in the contrary direction takes place; for as soon as the air in motion receives a check by the narrowness of the outlets, it rebounds up the workings towards the centre of motion, *i*, and continues retreating so long as the displaced air is kept in. But when, partly by an increased degree of pressure, and a diminution in the force of the explosion, the pressure is adequate to send the air through the outlets, the backward motion then ceases. It may not cease, however, till after two or more repetitions of this motion have taken place: one commencing at the outlets, as soon as a former has ceased at *i*. Though this returning motion tends generally towards the place of combustion *i*, it is carried in other directions by the workings: for, as soon as the returning motion has reached the end of the thirl *d*, it spreads into that working as well as down the working *g x*; and on arriving at the thirl 1 *q*, it enters this place while it continues down the working 2 *x*. Again this retrograde motion passes from B to the end of the first working, and then spreads into it also; and as it continues to move in the direction B *k*, it fills all the workings as it passes them. When the mine is thus filled with compressed air, a considerable force acts against the doors, and stops in the range B *a*, and against the doors between A and *h*. If these doors and stops are strong enough to support this pressure, all the heated air escapes through the two outlets only; but if the quantity of burning gas be great, the pressure will displace the weakest of them, which suppose the stop *f* and the door next the pit, and then the explosion will fly along the working A B, and also up the pit A, until the whole of the displaced air is expelled out of the mine. When the explosion is over at the top of the pit, a momentary stillness ensues; and then a quick current passes down the pit, but which decreases, and very soon becomes only the regular current of the air course.

In describing the reflected explosive motion I omitted to mention an important circumstance. When the explosion has struck one side of a working at an oblique angle, the air in motion leaves it at an angle which is a little less than that by which it approached the side. This I have shown before; but that part of the air which approached the working on the left hand of the explosion leaves the side on the right hand: hence an internal whirling motion is imparted to the blast every time it strikes the coal wall. This whirling motion collects the dust, and other light bodies, which the miners find floating in the air, on entering a mine soon after an explosion: and it is this motion that tosses the miner after he has felt the first contact; and which very often does him more injury, by bringing sharp stones or loose pieces of wood against him; and sometimes lifts him from the ground, and enables the direct motion to carry him forward against the sides of the working, or against pillars of fallen roof, props, or other exposed bodies. The undisturbed explosion, though pretty strong, does him very little comparative injury, if it gives him time to lie down, as it generally does by its thunder-like noise; for then it passes by without displacing him; and he has only to fear being injured by small loose pieces of wood or stone that it may possibly bring along with it.

The motion towards the place of combustion may happen under different circumstances. When the explosion reaches a forehead it rebounds again. When a great quantity of heat is suddenly given out by combustion in a confined place, a series of rapid retrograde movements is made in the immediate vicinity of the fire, until an adequate degree of pressure is obtained to force out the displaced air. The nature of this retrograde motion, which I have before described, may be illustrated by the following example. Suppose a sluice, having in its middle a small upright oblong aperture, which reaches from the top to the bottom, be put into a descending trough, through which there runs a body of water. The sluice prevents the most of the water from passing; but the water kept back obtains, in the following manner, a sufficient altitude to force itself through the aperture. It returns from the sluice with a small head, or a level, until it is lost in the ascent of the trough; this is no sooner done than a similar backward current commences at the sluice, and retires until it meets the descending current of water. These backward currents are repeated until the head obtained is sufficient to force the water through the aperture; but before they cease, too great an altitude is usually obtained, and then a few small currents run toward the sluice until the exact altitude is acquired. These currents of water are highly illustrative of the retrograde motions in coal-mines. Such motions in these mines also take place when the explosion has pervaded a great area of mine, if it be then pent in by a few narrow workings, as at the narrow openings *g* and B. Retrograde motions are also observed when the explosion is at first retarded by the doors and stops of ventilation; but they cease when it has obtained a sufficient pressure to displace these obstructions.

The mine is injured by the explosion, either by driving out of their situations such doors and stops as it meets with in traversing the mine, or by displacing them in obtaining an increased pressure. The impinging force is certainly more powerful, and does greater injury to the mine, where it acts only on a few doors and stops; but the pressure, by acting on a great number, exerts its force equally against the weak and the strong, so that those which are the least able to resist it are sure to be displaced. The displacement of stops and doors prevents the circulation of the atmospheric air, and the miners who cannot quickly escape out of the part affected by the fire are suffocated by the noxious gases of combustion. It is difficult to prevent the fatal effects of an explosion; as, should we know its place of departure, the course that it may take through the mine is not to be calculated with any degree of accuracy; as the smallest deviation in the situation of the side impingements will either make the explosion take this or that working. Thus, had the explosion struck the pillar *v* a little further on than *p*, none of it would have entered the working *p q*; or, had it reached the side of the pillar *w* 2 a little below *y*, it would have passed altogether through the opening 4, 5, and none of it have gone in the direction 1, 2. This uncertainty as to the direction which the blast may take makes the safety of the miners very precarious in any situation in the vicinity of a fire.

This description will suffice to show the effects of an explosion on the mine. I might have given a more extensive example, and carried the ravages of the explosion into other districts of the mine; but it would only have been a repetition of the same motions varied a little in every instance, according to certain differences in the affected parts of the mine.

This communication, Sir, concludes my Essays on Fire-damp; but I shall shortly have for your consideration an Essay on the Choak-damp of Miners.

I am, Sir, with great esteem,

Your humble servant,

John B. Longmire.

Article X.

On the Ancient Purpurissum; the State of Turkey Red Dyeing in the Greek Empire; and the Means of procuring a Substitute for Purpurissum.

(To Dr. Thomson.)

Sir,

Should the accompanying observations on the ancient purpurissum, on the state of Turkey red dyeing in the Greek empire, and on a probable means of procuring a substitute for purpurissum in the present day, appear worthy of a place in your *Annals*, they are much at your service, and I remain,

Sir, your most obedient humble servant,

Glasgow, Nov. 22, 1816. A CONSTANT READER.

I believe that a good and economical pigment of a carmine red colour has remained a desideratum amongst artists since the time when the knowledge of the method of preparing the purpurissum of the ancients became lost to the world. Our highest class of artists, the painters in oil colours, are precluded in a great degree from the use of carmine by the enormous expense of procuring it in quantity, and by its difficulty in mixing with oil. For these reasons, I believe, they are compelled to substitute for the use of carmine the yellower reds, such as vermilion, and ochre, and to reduce them to a tinge fit for the representation of the various lines of the human skin by a mixture of blue. It is evident, however, that this must prove but a dusky imitation of nature; and as detailing an experiment to procure a substitute for carmine, perhaps you may deem this letter worthy of a place in your *Annals*, although many of your readers may consider its contents of a trifling description. Chaptal supposes that the ancient purpurissum was obtained by dyeing the creta argentaria with a decoction of madder. This, however, I should conceive to be a difficult process; and one by which a pigment of a vivid red could not be obtained. Could such a colour, however, as what we now impart to cotton by the Turkey red process be given to a pigment, an object of value and importance would certainly be gained, by procuring a colour fit for the use of painters, indelible, and brilliant. A probable means of effecting this was suggested to me in reading Messrs. Kirby and Spence's work upon Entomology. The account given in that work of the nature and habits of the tinea tapetzella, or clothe's moth, impressed me with an opinion that the larvæ of this insect might be made an essential accessary in the preparing of purpurissum. Messrs. Kirby and Spence say that pigments of a brilliant hue are obtained from the excrements of the larvæ of the clothe's moth by feeding them on woollen cloth of the colour of the paint wanted. Now it struck me that, could these insects be nourished on cotton dyed Turkey red, their excrements might serve for preparing a colour superior to carmine. I accordingly procured some of the larvæ, and shut them up in a large jar, which I filled with cotton dyed Turkey red. At the end of a fortnight I opened the jar, and found the larvæ alive, though apparently more languid than when first shut up. They had, however, fed on the cotton, and their excrements were of a vivid red. I allowed the jar afterwards to remain shut for the space of four months, being too much busied by other avocations to think of attending to it. When I did open it, however, the larvæ were dead, and only one of them had undergone the transformation into the imago state. This one was also dead, and, although quite perfect, was of a wax-yellow colour. A considerable quantity of excrement, however, was deposited, which I dissolved in water, to which it imparted a portion of its red colour; but much of the cotton, although macerated into the most minute particles, had evidently suffered no material alteration in the stomach of the larvæ. Having, however, had it so far decomposed by the process as to have its colour reduced to a state in which water acted upon it as a solvent, my point was in a great degree gained; for by adding a solution of alum to the fluid, and precipitating the alumina from the mixture by an aqueous solution of soda, I am certain that I should have obtained a more elegant pigment than the creta argentaria dyed according to the process of Chaptal, narrated by him in his essay on the encaustic painting of the ancients. Could the larvæ of the clothe's moth be made to thrive on cotton, it is thus evident that any quantity of such a pigment might be procured in this way by artists, at hardly any expense or trouble. I regret, however, that the quantity I obtained was not sufficient to enable me to precipitate the colouring matter from the mixture; and the death of the larvæ, and more important concerns, have prevented me from repeating the experiment. I can account for the larvæ not thriving upon the cotton from the circumstance of that which I had used being full of the oil employed in the dyeing process: perhaps if this were remedied, and if the larvæ were removed from the woollen cloth where they had been hatched at a different season of the year, or at a different stage of their existence, this evil might be avoided.

Perhaps it will not be considered out of place if I subjoin some observations tending to illustrate an opinion which I have formed giving a higher antiquity to the process of dyeing with madder than the one usually entertained, and by which I have been induced to believe that the subjects of the Greek empire were acquainted, in the time of Justinian, with a method of dyeing silk, woollen, and linen, by means of madder. Independent of Chaptal's assertion, that the ancients knew how to dye creta argentaria of a red colour, Gibbon, in the seventh volume of his History, in quoting Cassiodorus, says, that the Phenician purple obtained from the murex was of a dark cast, as deep as bull's blood—"obscuritans rubens, nigrido sanguinea." He adds, that "the use of cochineal now enables us far to surpass the beauty of the colours of antiquity." In his History, vol. ix. p. 57, the same author says, that "an apartment of the Byzantine Palace was lined with porphyry, and reserved for the use of the pregnant empresses; and that the royal birth of their children was expressed by the appellation *porpherogenito*, or born in the purple. In the Greek language purple and porphyry are the same words; and as the colours of nature are invariable, we may learn, says the historian, that a dark deep red was the Tyrian dye which stained the purple of the ancients. Now we know that no species of the genera of vermes; murex, or buccinum, gives a colour at all answering their description; nor can the colour given by any limax of the present day be imitated by cochineal; on the contrary, the Syrian purple of Gibbon and Cassiodorus exactly resembles a colour easily made from cochineal; and if the colours of nature be invariable, the porphyry of the Byzantine Palace (probably the common oriental porphyry of a claret red), and the deep bull's blood red of Cassiodorus, particularly if, as Gibbon says, these colours could be imitated by cochineal, must have been much more similar to the colours dyed by Bancroft on silk and woollen by madder than to the shell purple extracted from the limax inhabiting the murex or buccinum.

ARTICLE XI.

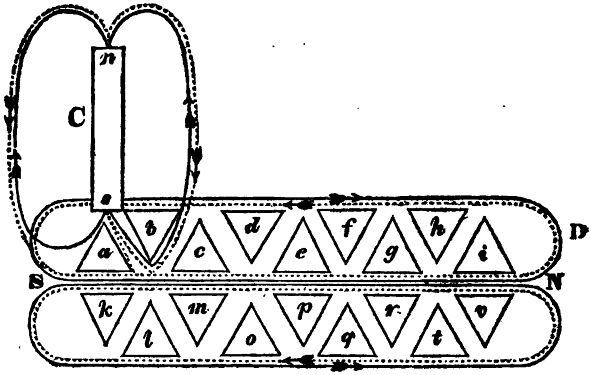
On Magnetism. By Mr. Andrew Horn.

(To Dr. Thomson.)

SIR, *High Wycombe, Dec.* 10, 1816.

AMONG those who have sought to develope the principles upon which a piece of iron or steel is converted into a magnet, and the laws of its phenomena, Epinus and Coulomb are particularly distinguished. The theory of Epinus, founded upon mutual attractions and repulsions between the magnetic fluid and particles of matter, is considered as defective, and has been superseded, at least in the estimation of the French philosophers, by that of Coulomb, who supposes two distinct fluids combined in the iron when in its natural state; but when the magnetic energy is induced, he conceives the two fluids to be disengaged from their state of combination, and, taking contrary directions, are accumulated at the extremities of the bar. Thus they exhibit actions analogous to vitreous and resinous electricity; the particles of each fluid repelling one another, while they attract those of the other fluid.

However, I presume that the two fluids, instead of being accumulated at the extremities of the magnet, *circulate* in contrary directions. On this presumption I shall endeavour to describe the process by which a piece of iron is brought to exhibit the magnetic virtue; and the result shall be the test of the theory. Let us conceive D to be a longitudinal section of a bar of iron or steel in its natural state; the moleculæ of which we shall suppose to be equilateral triangles, and arranged as *a b c d e f g h i k l m n o p q r s t v*, and the spaces between them to contain nearly equal proportions of the two fluids. Let us conceive another bar, C, possessed of the magnetic energy; that is, having its fluids *circulating*, the *austral* fluid issuing from *s* and again entering the bar at *n*; while its *boreal* fluid proceeds from *n*, and enters the other extremity at *s*; this fluid being represented by the dotted line, and the *austral* fluid by the continuous line. Let us now conceive the magnetic bar C placed upon the bar of iron D, in order to excite the magnetic energy in the latter; the immediate effect is, that as soon as C comes in contact with *a* in the bar D, the *austral* fluid issuing from *s* impels the *austral* fluid from the surface of *a*, and this portion is driven in the general circulation of C from S to N. The *austral* fluid of *a*, lying on the facet of *a*, next to *k*, is also affected, and rushes into the common circulation. While the *austral* fluid of C has produced this effect upon the *austral* fluid of *a* in the bar D, the *boreal* fluid of the latter undergoes a similar change from the circulating *boreal* fluid of C coming from *n*. Hence the *boreal* fluid belonging to *a*, in the spaces *a k* and *a b*, is impelled from S, and enters the bar C at *s* with its circulating fluid, and thus *a* becomes an element of the magnet C. Now while C is sliding over *b*, the austral fluid of *c*, lying on its surface in the space *c m*, rushes forward, to compensate the loss of the fluid from *a*, which is going into the general circulation of C. Thus, in the same manner as *a* became an element of the magnetising bar C, *b* and *c* also become elements. It is now easy to perceive how a continuation of the process will successively convert *d e f g h i*, likewise, into elements; so that when the magnetising bar is removed, there will be a complete circulation of the two fluids around the moleculæ *a b c d e f g h i*; the *austral* fluid will issue from S, and enter at N, while the *boreal* fluid issues from N, and enters again at S.



Let us suppose the magnet C again placed over the molecula *a* in the bar D; the consequence will be, that the *boreal* fluid of C issuing from *n* will impel the *boreal* fluid from the surface of *k* opposite to S, and circulating round *k*, will enter the magnet C, by the channel *a b*, at *s*; and thus *k* becomes an element of C. The *austral* fluid of the magnetising bar C, when sliding over the space *b d*, at the same time drives off the *austral* fluid lying upon the facet of *k* opposite to *a*, and also that on the facet of *l*, next to *k*, into the common circulation, and it enters C at *n*. The *boreal* fluid coming from *n* impels the *boreal* fluid on the other surface of *l*, and that also on its facet opposite *m*, and it is driven in the circulation through the channel *b c* to *s*. Thus *k* and *l* become elements of the magnetising bar. It is now evident how the remaining moleculæ *m o p q r t v* may in like manner be converted into so many elements of the magnetic bar; and that the two series of moleculæ *a b*, &c. and *k l*, &c. are entirely under its influence. But when the bar C is removed, the channels *a b*, *k b*, &c. will be stopped, the repulsions of the fluids at the angles in the oblique channels balancing each other. The fluids belonging to each series will only pass through the straight channel N S, and continue to circulate round their proper moleculæ.

We can now explain the cause of the surprising phenomenon, which presents itself when a magnetic bar is cut, or suddenly broken, so as to detach a portion from it; however short, that portion instantly becomes a magnet. This fact I consider as a serious objection to the theory of Coulomb; for on his hypothesis, if a bar were broken, we should not have two magnets differing only according to their size; but we should find the larger portion possessed of one pole extremely weak, while the other pole possessed all its original power. The *austral* fluid, in short, is nearly all confined to the one portion, and the *boreal* fluid to the other. M. Haüy endeavours to remove this difficulty by supposing that the fluid within the separated portions distributes itself to the extremities. But still there would be such an excess of *boreal* fluid in the one piece, and of *austral* fluid in the other, as would render the polar energy in each very sensibly different, which is contrary to experience. On the contrary, if we suppose any portion of the bar D broken off, we at once perceive how each portion is a complete magnet.

The cause also of what are called *consequent* points or poles of smaller power is well accounted for by this theory. These poles are situated in the oblique channels, and may arise from an irregular arrangement of the moleculæ, or from some error in the operation of magnetising the bar. In a future communication I shall explain by this theory a curious magnetic experiment related by the late Professor Robison.

I am, Sir, your obedient servant,
ANDREW HORN.

Article XII.

Analyses of Books.

Georgii Wahlenberg, Med. Doc. et Botanices Demonstrat. in R. Acad. Upsal. R. Acad. Scient. Stockholm Membr. Ord. Flora Carpatorum Principalium exhibens Plantas in Montibus Carpaticis inter Flumina Waagum et Dunajetz eorumque ramos Arvam et Popradum crescentes, cui præmittitur Tractatus de Altitudine, Vegetatione, Temperatura et Meteoris horum Montium in Genere. Göttingæ, impensis Vandenhöek et Ruprecht. 1814.

We are indebted to Dr. Wahlenberg for a most interesting account of the climate and vegetation of Lapland, and an equally curious dissertation on the mountains of Switzerland. The present work is no less entitled to our attention. The Carpathian mountains constitute an elevated chain at a great distance from the sea, and form the boundary between the flat countries of Poland and Hungary. Hence it is probable that they have considerable influence on the meteorological phenomena of Europe. They are not so high as the Alps of Switzerland and Scandinavia; but their central situation give them no less a claim to our attention. Hitherto the heights, the structure, and the vegetation of these mountains, have been very imperfectly known; but Dr. Wahlenberg has in a great measure filled up the gap which existed by the singular industry with which he examined these mountains. He went to Vienna in May, 1813; thence, supplied with the proper letters, he went to the neighbourhood of the Carpathian mountains, and ascended the Fatra, the furthest west of them, on June 11. He continued exploring the different mountains till Aug. 24, when a violent inundation laid the country under water, and confined him at Kesmark till Sept. 2. On Sept. 5, he renewed his excursions into the mountains. On Sept. 11, the inundations were repeated, and the bridges again destroyed; but he was able to renew his labours on Sept. 14, and to continue them till Oct. 17, when the country was so completely covered with snow that all hopes of further investigation were at an end.

On this he went to Buda, in order to determine from the barometrical observations kept there, the height of the observatory above the sea, conceiving that the heights of the Carpathian mountains could be determined more accurately by a comparison of the heights of the barometer which he observed with corresponding ones at Buda, than by a comparison with observations made at a greater distance; for he has shown by tables that the variations of the barometer on the Carpathians correspond better with those made at Buda than at Vienna, which is further distant. He then went to

Vienna, where he had the means of obtaining more accurate information respecting several of the subjects which he was investigating. I shall now endeavour to lay an abstract of Dr. Wahlenberg's introductory dissertation before my readers:—

His barometrical measurements are founded upon the following data, which he considers as established by his own observations:—

1. The more mutable the temperature of the air in any country is, the greater are the variations of the barometer in it.
2. The barometer oscillates most at those seasons of the year when the temperature varies most.
3. The barometer oscillates more in high mountainous regions than in great plains.
4. In general the mercury of the barometer falls in rainy and cold weather.

The height of the observatory at Buda above the level of the sea is 508 English feet.

The following table exhibits the elevation of the Danube above the level of the sea:—

At Vienna	446½ Eng. feet
Presburg	330
Raab	272
Buda	229

The elevation of the observatory at Vienna above the sea is 566½ English feet.

The Carpathian mountains are situated between N. lat. 48° 55′ and 49° 15′, and extend about 1½° of long. The furthest west is denominated Fatra. It runs north and south, and is divided into two by the river Waag, which passes through it on its way to the Danube. The following peaks are the highest of this mountain measured by Wahlenberg:—

Coch	5196 Eng. ft.
Krivan	5648 (about)
Klukberg	4442
Czerny-kamen	4583 (about)

From Fatra there runs a chain of mountains east, terminating in the great Carpathian Alps. Of these the only one of considerable height, before approaching the eastern Alps, is Chocs, the elevation of which above the sea is 5236 feet. About 12 miles east from Chocs the great Eastern Alps begin. This chain can in fact be considered only as one great mountain about 24 miles in length, and 10 miles in breadth. Towards the west the high portion is much narrower than towards the east, and the highest elevations of all are towards the eastern end. This immense mountain mass contains various plains and valleys, and a good many lakes are to be found in it. To this mountain the inhabitants of the country have given the name of Trata (*hideous*), from its singular and dreary aspect. The following are the heights of the most remarkable peaks belonging to this mountain, as measured by Wahlenberg:—

Krivan	8034	Eng. ft.
Nod Pavlova	5942	
Nochstein	4984	
Viszoka	8313	(about)
The lake Hinzka	6219	
Csabi	8313	(about)
Gerlsdorfkessel	7780	(about)
Great Lomnitzerspitze	8464	
Hundsdorferspitze	8313	(about)
Rotheseethurm	7673	(about)
Hintere Leithen	6591	
Stirnberg	6287	

On the south side of the Waag there is a smaller alpine chain running nearly parallel to the great Tatra, the highest part of which, called Djumbier, is 6576 feet above the level of the sea.

From the imperfect account which Wahlenberg gives of the structure of these mountains, it appears that the central and highest parts of them are primitive, consisting of a granite composed of quartz and milk-white felspar with very little mica. At a lower level, transition rocks make their appearance, consisting of greywacke and transition lime-stone. The lime-stone is most abundant on the north side of the mountains, and seems to be nearly wanting on the south side. It appears probable that floetz rocks make their appearance in these mountains lying over the lower portion of the transition rocks; at least Wahlenberg speaks of a sand-stone slate, which he says is very different in its appearance from the greywacke which he had before noticed.

The vegetation on the Carpathian mountains differs considerably from the vegetation on the northern Alps of Switzerland. The Austrian botanists ascribe the great number of plants which occur in their country to the lime-stone rocks on which they vegetate; but Wahlenberg does not think that the remark is just as far as applies to the Carpathian mountains. He found very few plants, indeed, confined to the chalk; and even these few, he thinks, owe their locality to some other circumstances.

Corn and fruit-trees grow and flourish at a greater height on the outskirts of the Carpathian mountains than in Switzerland.

The *woody region*, or the region of beeches, is richer in plants than the same region in the Alps of Switzerland. The termination of the beeches he places at the height of 4194 English feet above the level of the sea, or a very little lower than in Switzerland.

The *subalpine region*, situated between the termination of the beech, and that of the pinus abies, or Scotch fir, exhibits nearly the same plants as in Switzerland; namely, acer pseudoplatanus, sambucus racemosa, &c. But as we ascend, a striking difference takes place. The gloomy and useless *pinus mughus* begins to cover the earth at an elevation of 4476 feet, and the termination of the pinus abies may be placed at 4902 feet above the level of the sea. In this respect there is a striking difference between the Carpathian mountains and the Alps of Switzerland. In these last the pinus abies vegetates to the height of 5862 feet above the level of the sea, or 960 feet higher than on the Carpathian mountains.

The *lower alpine region*, or the region of the *pinus mughus*, extends from the termination of the pinus abies to where the mughus reaches only the height of two feet in open places. This region is very natural in the Carpathian mountains, and much more easily determined than in Switzerland or Lapland, in the former of which indeed the alnus viridis, and in the latter the salix glaucus, exist; but they are too thinly scattered to point out the exact boundary with precision. On that account Wahlenberg was obliged to make use of the lower snow line to mark the limit of the lower alpine region in these mountains. But the great abundance of mughi on the Carpathian mountains furnishes an excellent means of determining that limit. It abounds in large and fine plants, which vegetate under the protection of the mughus; such as the doronicum austriacum, cortusa matthioli, cineraria crispa, hypochæris helvetica, swertia perennis, polygonum bistorta, &c. which ascend to a greater height under the cover of the mughus than they do in the Alps of Switzerland. In places destitute of mughus the earth is barren, being covered with poa disticha, with plants of campanula alpina, senecio abrotanifolius, &c. exhibiting in a remarkable degree the nature of the Carpathian mountains. The upper boundary of the mughus is 5968 feet above the level of the sea. Below this level almost the whole soil is covered with this dismal shrub. Above it the plant is observed here and there creeping among the stones to the height of 6394 feet above the level of the sea. But these plants are of little consequence, and far from conspicuous.

The *superior alpine region* commences where the boundary of the mughus is placed, or 5968 feet above the level of the sea, and continues to the highest peak of the Carpathians. It can be observed only in perfection on the Tatra. All large vegetables disappear when we come to this region. The Tatra exhibits a dry and barren aspect, which reminds one of the dry appearance of some parts of Lapland. But upon a nearer inspection the resemblance does not hold. In Lapland it is the subalpine region which hurts the eye on account of its barrenness, owing to the heathy and similar plants with which the whole soil is covered; while in the Carpathian mountains these plants are wanting. The alpine region in these mountains has a most barrent aspect, being in great part covered with naked stones. The little soil that exists produces poa disticha and senecio abrotanifolius, scattered among which we perceive short plants bearing large flowers, such as arnica doronicum, primula minima, campanula alpina, gentiana frigida, dianthus alpinus, and serratula pigmæa which is the most singular of them all. It is the largest plant which is to be found in these high regions, and was found by Wahlenberg most abundantly on Kahlbachergrat at the height of 7141 feet above the level of the sea. These plants in general differ from other alpine plants by their spongy nature. Their spindle-shaped roots look like a sponge with vascular threads passing through it. The alpine region reaching from the boundary of the mughus to the snow line, an interval of 2558 feet, or extending to 8526 feet above the level of the sea, may be divided into two parts. The lower portion of it contains the above-mentioned plants, together with empetrum, vaccinium uliginosum, and salix retusa; but the upper portion, beginning at 6927 feet above the level of the sea, is quite barren. The cliffs and rocks are more destitute of plants than any alpine regions previously seen by Wahlenberg; and, what is singular, they are no less destitute of snow. Nothing is to be seen but naked cliffs, or heaps of loose stones, destitute of all other vegetation except black lichens, with which they are covered.

It is not less singular that the snow line should be so much higher on the Carpathians than the Alps. Mons Pilatus, in Switzerland, only 6927 feet above the level of the sea, is covered with perpetual snow; whereas not one of the peaks of the Carpathian mountains is covered with perpetual snow, though the great Lomnitzerspitze is 8464 feet above the level of the sea. Snow lies, indeed, during the whole year in some of the gullies and chasms of these mountains, and there is a kind of a glacier at Eisthalerspitze, owing to this cause. Wahlenberg is disposed to place the snow line on the Carpathians at the height of 8526 feet above the level of the sea, which is higher than any of the Carpathian mountains. This superior elevation of the snow line in these mountains, notwithstanding their being in a higher latitude than the Swiss Alps, Wahlenberg considers as owing to the prevalence of the hot winds from the plains of Hungary; for Hungary exhibits by far the greatest plain in Europe, and it lies so far to the south that the heat in summer is very considerable. This will be evident from the following table, exhibiting the annual temperature at the Buda observatory drawn up by Wahlenberg from the observations kept at that place. The degrees are those of the centigrade thermometer; but I have added a column, exhibiting the mean of the months according to Fahrenheit's scale, for the accommodation of the reader:—

Months.		Mean from the highest points.	Mean from the lowest.	Mean of the two preceding.	Monthly mean.	Do. according to Fahrenheit's scale.
Jan.	1 to 10	−0·37°	−2·72°	−1·54°		
	11 to 20	−1·34	−3·86	−2·60		
	21 to 31	−1·44	−5·09	−3·26	−2·46°	27·6°
Feb.	1 to 10	1·48	−1·85	−0·18		
	11 to 20	2·70	−0·11	1·29		
	21 to 28	3·41	−0·32	1·54	0·88	33·6
Mar.	1 to 10	4·18	0·24	2·21		
	11 to 20	5·32	1·76	3·54		
	21 to 31	7·07	2·70	4·88	3·54	38·35
April	1 to 10	9·15	4·17	6·66		
	11 to 20	12·58	7·50	10·04		
	21 to 30	14·19	9·59	11·89	9·53	49·15
May	1 to 10	18·32	13·66	15·99		
	11 to 20	21·87	16·91	19·39		
	21 to 31	21·97	16·89	19·43	18·27	64·88
June	1 to 10	22·91	18·05	20·48		
	11 to 20	22·52	17·92	20·22		
	21 to 30	22·27	17·71	19·99	20·23	68·41
July	1 to 10	22·57	18·19	20·38		
	11 to 20	24·52	20·10	22·31		
	21 to 31	25·41	20·35	22·88	21·86	71·35
Aug.	1 to 10	25·02	19·90	22·46		
	11 to 20	24·19	19·15	21·67		
	21 to 31	24·64	19·52	22·08	22·05	71·69
Sept.	1 to 10	22·17	16·90	19·53		
	11 to 20	19·39	14·42	16·90		
	21 to 30	17·41	12·50	14·96	17·13	62·83
Oct.	1 to 10	15·42	10·79	13·10		
	11 to 20	13·13	8·67	10·90		
	21 to 31	12·05	8·19	10·12	11·37	52·46
Nov.	1 to 10	8·58	5·39	6·98		
	11 to 20	5·66	2·92	4·29		
	21 to 30	5·07	2·69	3·88	5·05	41·09
Dec.	1 to 10	2·79	0·45	1·62		
	11 to 20	−0·45	−2·71	−1·58		
	21 to 31	0·41	−2·11	−0·85	−0·27	31·5

The mean of the whole year is 51·076° Fahrenheit, or not more than 1° higher than the mean temperature of London; but the mean of Aug. is 9½° higher at Buda than at London. Wahlenberg compares the temperatures at Buda and Turin during the years 1811 and 1812, the one of which was very hot, and the other very cold. The following table exhibits the comparison. I have reduced the degrees to those of Fahrenheit, for the sake of the English reader, to whom these degrees are much more familiar than those of the centigrade thermometer used by Wahlenberg.

	1811.	1812.	Difference.
Mean of the whole year at Buda	52·22°	50°	2·22°
Ditto at Turin	50·68	43·9	6·78
Mean of the summer at Buda	75·16	69·53	5·63
Ditto at Turin	66·56	61·83	4·73

Dr. Wahlenberg kept a register of the height of the thermometer at Kesmark, from Aug. 11 to Oct. 11, 1813. The following table exhibits a comparison of these observations with those at Buda :—

		Kesmark.	Buda.	Difference.
Aug. 11 to 31	Mean of the highest points ..	62·09°	69·42°	7·33°
	Ditto lowest	50·52	60·64	10·12
Sept. 1 to 20	Ditto highest................	57·56	64·02	6·46
	Ditto lowest................	44·33	56·14	11·81
Sept. 21 to Oct. 10	Ditto highest................	50·16	57·88	7·72
	Ditto lowest..	40·06	49·85	9·79

We see from this table that there is a much greater difference between the temperature of Kesmark and Buda during the night than during the day. The mean of the difference between the lowest, or nocturnal temperatures, is 10·57, while the mean of the difference between the highest, or diurnal temperatures, is only 7·17. The mean of all the differences is 8·87. Probably the mean temperature of Kesmark is so much lower than the mean temperature of Buda.

Wahlenberg made some observations on the mean temperature of the earth in the Carpathian mountains by determining the heat of springs in different places. The following are the results which he obtained.

Two fountains in the valley of Lubochna, at the height of 1781 feet above the level of the sea, had the following temperatures :—

July 8, 1813	45·14°
26	45·32

A fountain a little higher up, issuing from the bottom of the mountain, was as follows :—

July 8, 1813	44·6°
26	44·6

On the other side of the valley, almost at the same height, another fountain was found of the following temperature :—

July 27	45·14°

These springs give undoubtedly the temperature of the earth, and show that at the height of 427 feet above the limit of the walnut-trees this temperature is not lower than it is in Switzerland at that limit; for the temperature of the earth in Switzerland at the limit of the walnuts does not exceed 44·6°.

At Botza, at the height of 3617 feet above the sea, an excellent and well-covered fountain was of the following temperature :—

July 20	40·1°
Aug. 11	40·28

A few miles west from Botza there is a fountain, called Weissbalden, 3815 feet above the level of the sea, the temperature of which was as follows:—

July 29	40·46°

From these two fountains we learn that the temperature of the earth on the Carpathian mountains, about 266 feet below the limit of the beech, is 2·7° lower than in Switzerland; where Wahlenberg found the temperature of the earth at the limit of the beeches to be 42·8°.

Below the cottages at Krivan issue the fountains called Dreybrunnen, 3556 feet above the level of the sea. Their temperature was—

June 30	40·64°
Aug. 10	41·36

The temperature of the fountain called Doszila, in the valley of Raczkova, 3901 feet above the level of the sea, was—

Aug. 3	40·28°

The temperature of a fountain at the upper part of the valley Stavnicza, in the Djumbier, situated 5219 feet above the level of the sea, was—

June 21	38·66°
Aug. 1	39·02

Another fountain, called Räuberbrunnen, 6176 feet above the sea, and situated on the upper side of the Djumbier, was of the temperature—

June 21	37·94°
Aug. 1	38·3

The Fischsee, which is situated at the height of 4807 feet above the sea, contains fishes, namely, the salmo fario. These fishes are to be found, likewise, in another lake at nearly the same height. All the other lakes in these mountains, which are higher, are destitute of fishes.

The inhabitants of Hungary complain of the great cold of the nights, even during the hottest weather; but the thermometer does not indicate any great depression. Wahlenberg ascribes the extreme effect of the evenings in that country upon animals to the great dryness of the winds, which, blowing over a large tract of continent, acquire the property of absorbing moisture with avidity; and the great evaporation in his opinion occasions the feeling of cold of which the inhabitants complain; but this explanation does not tally with the great deposition of dew which he acknowledges takes place there, and which we know takes place likewise in Egypt and Arabia, and other countries where a similar sensation of cold is perceived. I have no doubt that the feeling is owing to the great quantity of heat radiated from living bodies during the night, in consequence of the almost perpetual cloudless state of the sky. It may be laid down as a general rule that the mean temperature of cloudy countries is much higher than might be expected from the temperature of the summer. Thus the mean temperature at London is only 1° lower than at Buda, though the mean heat of August at Buda is $9\frac{1}{8}$° higher than at London. Von Buch points out the same remarkable circumstance as belonging to the North Cape. It holds also in Iceland. The mean temperature of the year in these countries is high, considering their situation; but the mean summer temperature is very low. The clouds serve to confine the heat in winter; while they hinder the solar rays from producing their full influence in summer.

Wahlenberg's Flora of the Carpathian mountains contains 1346 species. Of these 1042 are phenogamous plants, the rest cryptogamous. He was under the necessity of omitting the fungi altogether, as he could neither examine nor preserve them.

To give the reader a correct idea of the Carpathian mountains, we have given an engraving of an outline of them drawn by Wahlenberg himself, for which we are indebted to a celebrated traveller and geologist at present in London.

ARTICLE XIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Saturday, Nov. 30, the Society held its annual meeting for the election of the office-bearers for the ensuing year. There were elected—

PRESIDENT—Right Hon. Sir Joseph Banks, Bart. G. C. B.

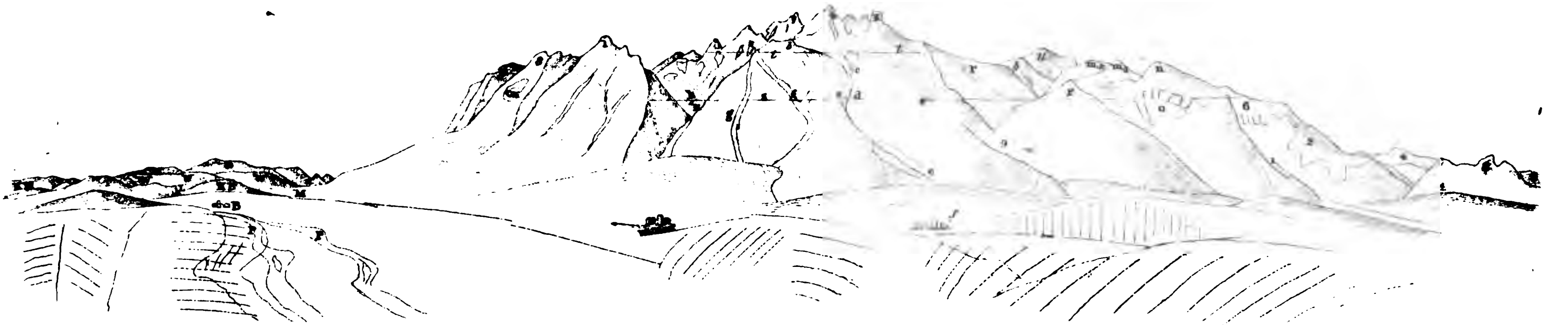
SECRETARIES—William Thomas Brande, Esq.
Taylor Combe, Esq.

TREASURER—Samuel Lysons, Esq.

There remained of the old Council—

Sir Joseph Banks, Bart.
John Barrow, Esq.
Taylor Combe, Esq.
Sir Humphry Davy,
Sir Everard Home, Bart.
Samuel Lysons, Esq.
The Earl of Morton,
John Pond, Esq.
William Hyde Wollaston, M.D.
Thomas Young, M.D.

Plate LXI.



K.H. Krolowa hola
T. mountains of Teplitz.

L. Luesivna.
B. Botzdorf.
K.B. Ken mountain before Luesivna.
M. Minksdorf.
P. Poprad river between Hundsdorf & Kesmark.
W.W.W. mountains near Schwartzenberg
M. Miskowa.

D. Djambier.
8. Gerlsdorf mountain with the cauldron of Gerlsdorf.
I. Mountain of Botzdorf.
i. Point of Schlagendorf.
h.h. Valley of Great Kalbach.
7. Point of Eisthal & Limits of the Mugho.

S.S. Limits of the Abies.
g. a chasm.
a. Point of Lomnitz.
K. Point of Hundsdorf
b. Saddle of the Ridge of Kalbach
c. Alpform of Lomnitz.
f. Little Schlagendorf.

2. Place where Mr. Wahlenberg passed the night of the 10th of July.
C. Lake of Trichter.
r. Mountain Ratzen.
9. Weiße Wand.
6. Durlsberg.
11. Thörichter Gern mountain or the fleischbanks.

P. Stöschen mountain.
M.1. the Leiten.
M.2. the hinter Leiten.
n. Sternberg.
o. Drechselhauschen.
1. Rayin.
6. Feinblöße.

2. Nesselblösse.
3. mountain of Landeck.
44. Mugguera.

View of the Carpathian mountains, taken from Durrenberg near Kesmark by G. Wahlenberg

There were elected into the Council—

William Thomas Brande, Esq.
John George Children, Esq.
John Wilson Croker, Esq.
Charles König, Esq.
Alexander Macleay, Esq.
Alexander Marcet, M. D.
Colonel William Mudge,
William Haseldyne Pepys, Esq.
The Earl of Spencer,
Sir John Thomas Stanley, Bart.

Twenty members have died since the last anniversary, and 32 new members have been admitted into the Society. The present list of Fellows contains 649 names. Of these 44 are foreign members.

On Thursday, Dec. 5, a paper by Mr. Tod was read, giving an account of some experiments made on torpedos at Rochelle. The object of the experiments was to ascertain whether the animal possesses a voluntary power over its electrical organs. When the fish is held by the tail, the person holding it does not receive shocks, nor are they communicated when the animal is held by the anterior part of the body. The electric shocks were given without any apparent diminution when an incision was made round the electric organs, and even when they communicated with the rest of the animal only by the nerves. When a portion of the electric organ was cut off, the strength of the shock was diminished; but Mr. Tod was not certain whether this diminution was owing to the diminution of the organ, or to the exhausted state of the fish. The nerves of the electric organs are supplied from the medulla oblongata. When Mr. Tod was cutting the electric organs, he received shocks through the scalpel.

The author states a circumstance respecting the torpedo which he has been told, he conceives, on good authority, though he never witnessed it himself. Where torpedoes abound, boys are in the habit of playing the following trick to those who are not in the secret. They persuade the ignorant boy to make urine upon the torpedo. The consequence is, that an electrical shock is conveyed along the stream of urine.

At the same meeting a paper by Mr. Hatchett was read, describing a method of destroying the musty taste in grain. Must, the author conceives, is an alteration which is produced in the amylaceous part of the grain, and in general it is confined to the surface of the corn immediately under the husk. To remove it, the corn must be put into any vessel capable of holding thrice the quantity of corn put into it. The vessel is then to be filled with boiling hot water, and the liquid allowed to remain till it be cool. Then the light and rotten grains, which swim on the surface, may be skimmed off, and the water allowed to drain. It will be proper afterwards to pour some cold water on the grain, and to stir it about in order to wash away completely the water which holds the must in solution. Grain thus treated will be found quite free from all musty taste. In a year like the present, when so much of the corn has been injured by wet, this information must be of great importance to the country.

On Thursday, Dec. 12, a paper by Mr. Brande, on an astringent substance from China, was read. It was given to Mr. Brande for examination by Sir Joseph Banks. It consisted of vesicular bodies like nutgalls adhering to the smaller branches of a tree. Insects could be perceived in it. There is a description of it by Duhalde, who says, that it varies from the size of a nutgall to that of a chesnut. Mr. Brande found its constituents as follows:—

Tannin and gallic acid	75
Resin	2
Woody fibre	23
	100

The tannin was of a brownish-yellow colour, and had an astringent taste. It was completely soluble in water and in alcohol of 0·820. It threw down peroxide of iron of a deep black from acids. The author tried to separate the gallic acid from the tannin by Davy's process; namely, digesting the solution with barytes, filtering and then throwing down the barytes by sulphuric acid. But this process did not succeed. He succeeded better when he digested lime in the solution, filtered, and then threw down the lime by oxalic acid. But even this process did not give pure gallic acid.

Mr. Brande found his tannin very soluble in alcohol of the specific gravity 0·820. He, therefore, draws as a conclusion, that the previous statement of chemists, that tannin is insoluble in absolute alcohol, is inaccurate. Had he consulted my System of Chemistry (vol. ii. p. 392), he would have seen that I had long ago ascertained the solubility of tannin in alcohol of the specific gravity 0·818, which is an alcohol containing only $\frac{1}{10}$ of its weight of water. But this is no reason for refusing to admit the accuracy of Richter's experiment, that tannin is insoluble in alcohol of the specific gravity 0·796.

Mr. Brande found, as had been done long ago by Scheele, that when nutgalls are distilled a quantity of the gallic acid comes over undecomposed.

On Thursday, Dec. 19, a paper by M. Dupin was read, on the Improvements lately introduced into Ship-building by Mr. Seppings. The author, in order to obtain materials for his projected work on ship-building, had been induced to visit Great Britain; and he expressed himself in the highest terms of the reception he met with from those gentlemen to whom he had occasion to apply. He stated a number of historical facts to show that the principle upon which Mr. Seppings's plan is founded had been previously known and employed in France, though afterwards abandoned. But he allows that Mr. Seppings has introduced so many improvements, and has so happily got over the difficulties to be overcome, as to have made his method in a great measure his own.

On Thursday, Jan. 9, 1817, part of a paper by Sir Humphry Davy on Flame was read. The author divided his subject under four heads:—1. On the effect produced by rarefaction by means of the air-pump on the inflammation of gases. A small jet of hydrogen gas from a glass tube was extinguished when the air was rarefied six times. But when the jet was larger it was not extinguished till the rarefaction amounted to 10 times. In the second case the point of the tube from which the gas proceeded was white hot, and the gas continued to burn till the tube ceased to be visibly red. It immediately occurred to the author that the cause of the extinction was not the deficiency of oxygen, but the want of sufficient heat. Hence it followed that those bodies which produce most heat, and which require the least for combustion, would burn the longest; and a set of experiments made on purpose confirmed these ideas. Hydrogen burned till the atmosphere was rarefied 10 times; olefiant gas, till the rarefaction was nearly as great; carbonic oxide was extinguished when the rarefaction amounted to five times; and carbureted hydrogen when it was only four times. Sulphur continued to burn till the rarefaction was 30; phosphorus, till it was 60; and phosphoreted hydrogen gas burned in the best vacuum which he could form by means of his air-pump.

The heat produced by the different gases when burning was found to follow the same order as the rarefaction in which they would burn. Hydrogen produces most heat, olefiant gas the next, then sulphureted hydrogen and carbureted hydrogen, and carbonic oxide the least of all. Carbonic oxide, being combustible at a much lower temperature than carbureted hydrogen, burns in an atmosphere more rarefied.

A mixture of oxygen and hydrogen gases does not explode by electricity when rarefied 18 times; but a mixture of chlorine and hydrogen still burns, though very feebly, when rarefied 24 times. When the rarefied mixture of oxygen and hydrogen is strongly heated, it then becomes capable of exploding by electricity; but only the heated portion burns.

2. On the effect of rarefaction by heat on the combustibility of the gases. Grotthus has stated that, when gaseous mixtures are rarefied four times by heat, they cease to explode. Our author was able only to produce an expansion of 2½ times. It was produced by a cherry-red heat; which of course indicates a heat of about 1032°. The result of his experiments is precisely the reverse of that of Grotthus. He found that rarefaction by heat increases the explodability of gaseous mixtures. He infers, likewise, from his experiments, that the hypothesis of Dr. Higgins, Berthollet, &c., that the reason why gaseous bodies explode by electricity is the compression occasioned by the sudden expansion of the heated portion

of gas, is erroneous. He considers the heat evolved by the combustion as the sole cause of the explosion.

On Thursday, Jan. 16, Sir H. Davy's paper on Flame was concluded. In the third part of his paper the author treats of the effect of different mixtures of other gaseous bodies on the combustibility of exploding compounds by the electric spark. He made a mixture of two volumes hydrogen and one volume oxygen gas, and tried the effect produced by adding various mixtures of other gaseous bodies. Olefiant gas was found to have the greatest effect in preventing the explosion of this mixture by electricity. The quantity of each gas necessary to prevent the explosion was different. From his experiments it appears that the effect does not depend upon the specific heat or the specific gravity of the gas added. He is of opinion that it depends chiefly upon the property of the gas to conduct heat. Gases, he thinks, differ as much in their conducting powers as solid bodies, and those which conduct best will act most powerfully in preventing explosion, by carrying off the heat, and cooling the mixture below the exploding point.

The fourth part of the paper consisted in general remarks and practical inferences. He finds that neither the rarefaction nor condensation of common air produces much effect upon flame burning in it. The effect of wire-gauze in preventing explosions he considers as owing entirely to its property of carrying off the heat, and thus reducing the temperature of the gases that pass through it below the exploding point. He gave an account of various improvements introduced of late into the construction of the safe lamps for coal-mines, and pointed out advantages arising from the yielding nature of the wire-gauze, of which they are constructed.

On Thursday, Jan. 23, a curious paper by Sir H. Davy was read, constituting an important addition to his preceding memoir. He had concluded from his former investigations that flame consisted of gaseous bodies heated above whiteness; and he had found that oxygen and hydrogen, as well as oxygen and charcoal, might be made to combine silently at a temperature below redness, and to form respectively water and carbonic acid. It occurred to him that during these combinations heat was given out, and that, though not sufficient to cause the explosion of the gaseous mixture, it might, notwithstanding, be able to heat a metallic body to redness. While thinking of an experiment to determine this point, the phenomenon exhibited itself accidentally while he was making an experiment with a safe lamp in a mixture of carbureted hydrogen and air. He plunged the lighted safe lamp into this mixture, and then caused an additional quantity of carbureted hydrogen to pass into the mixture. The lamp was extinguished; but a platinum wire that was above the flame became red-hot, and continued so for several minutes; and when it ceased to be luminous, the mixture had entirely lost its exploding properties. It was immediately obvious that the heat was evolved by the silent combination of the carbureted hydrogen with the oxygen of the mixture; and that, though not capable of exploding the mixture, it was yet capable of heating the platinum to redness. On making exploding mixtures of oxygen with hydrogen, and other inflammable gases, and plunging a hot platinum wire into them, he found that it became red hot, and continued so till the mixture had lost its power of exploding. Vapour of ether, alcohol, or naphtha, mixed with air, had the same property. He describes an experiment which every person can make, and which serves admirably to illustrate the fact. Let a drop of ether fall into a glass vessel, heat a platinum wire by means of a hot poker, and plunge it into the vessel. It will immediately become red-hot in some part, and continue so till the ether is consumed. During this silent combustion of the vapour of ether there is a phosphorescent light connected with some curious chemical changes which take place in the ether, and which he is at present engaged in investigating.

Platinum answers best for these experiments, on account of its small capacity for heat, and its small radiating power. The author tried silver, copper, and iron, but did not succeed with any of them. But as the wires of these metals were not very small, he does not consider the point as decided by the experiments which he has hitherto made. He terminated his communication with a practical application to coal-mines. If a wire of platinum be suspended over the flame of a safety lamp properly coiled up, and if the lamp be taken into an exploding mixture, it will be extinguished, but the platinum wire will become red hot, and will continue to give out light till the mixture loses its exploding qualities. By this light the miner may direct his way out of the exploding mixture.

I consider this as one of the most beautiful discoveries which Sir H. Davy has made. The numerous practical applications of it to gaseous experiments must be obvious to chemists in general.

At the same meeting a paper, by Dr. Brewster, on Light, was read. This paper consisting of a great number of detached facts, it is difficult to give any account of it. He showed how the metals by their polarization of light form the supplementary colours. He stated also that common salt and fluor spar, when in pieces large enough, act upon light in the same way as doubly-refracting bodies.

LINNÆAN SOCIETY.

On Tuesday, Dec. 3, a description was read of a fossil belemnite in flint, by Dr. Arnold. The specimen was remarkable, because it exhibited a very distinct jointed syphunculus passing through the fossil. Very little is known respecting the nature of the animal that inhabited this fossil. Dr. Arnold conceives that it was capable of rising or sinking in water at pleasure, and that its structure was somewhat similar to that of the nautilus or cornu ammonis.

At the same meeting several specimens of an unknown fossil in flint, sent by Dr. Arnold, were exhibited. They consist of small flat spherical bodies, having a depression in the centre, in which is a small tubercle, so as to give an appearance somewhat similar to a small acorn before it is ripe, and while still in its cup. Each of these spherical bodies sends a vessel into each of the spheres that surrounds it; so that the fossil resembles a kind of net-work. The usual size of the spheres is rather less than a peppercorn, and the vessels are as fine as hairs. No name has hitherto been given to this fossil.

At the same meeting a specimen of an unknown fungus from Virginia, sent to the Society by Dr. Mitchell, was exhibited. It was very heavy, white, roundish, had a starchy smell, and when burned gave out no animal odour. It was probably some tuber rather than a fungus.

At the same meeting the remainder of Mr. Beechino's paper on the British junci was read.

On Tuesday, Dec. 17, a paper by Dr. Arnold was read, giving a description of a remarkable volcanic mountain in the island of Java. Dr. Arnold paid a visit to this mountain, and drew up his description of it on the spot. It is called by the natives Tankubanprau. The road to it is very difficult, being through an almost impenetrable jungle. The crater has nearly the form of a truncated cone inverted. The sides are about 500 feet high, and in many places nearly perpendicular. There is a small lake at the bottom filled with water, having the taste of a solution of sulphuric acid. This water was boiling in several parts of the lake. But its temperature at the edge, taken by Dr. Horsfield, was 112°. It was surrounded by a soft mud, apparently a mixture of sulphur and clay. Dr. Arnold is of opinion that it occasionally emits flames, for the trees round its edge had the appearance of being scorched. On the west side of this crater, and merely separated from it by a thin diaphragm of rocks, is another crater, rather larger than the other, and having at its bottom a lake of cold water. From this circumstance Dr. Arnold concludes that the two craters, though so near each other, had not any connexion.

On Tuesday, Jan. 21, a paper by Sir James Edward Smith was read, on the Genus of Plants called Tofieldia. He described six species of this genus, the first five of which had hitherto been confounded together by botanists under the Linnæan name *anthericum caliculatum.* These he called

Tofieldia palustris, a native of Scotland,
alpina, a native of Switzerland,
stenopetala,
cernua.

ROYAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year 1815.

MATHEMATICAL PART.—*By M. le Chevalier Delambre, Perpetual Secretary.*

MEMOIRS APPROVED BY THE CLASS.

ANALYSIS.

(*Continued from* vol. viii. p. 462.)

On the refractive and dispersive Powers of some Liquids, and the Vapours which they form. By MM. Arago and Petit.

The theory of refraction is one of the most important parts of optics, and all the philosophers, who have explained or supported the different systems contrived to account for the phenomena, have particularly endeavoured to connect the law of refraction with the hypothesis which they admitted.

Newton, in ascribing refraction to an attraction of bodies for light, has given so natural and clear an explanation of this phenomenon, and of its laws, that it has been always considered as one of the principal arguments in favour of the system of emission. However, of all the general consequences deduced from this hypothesis, the only one verified hitherto is the law of the constant ratio of the sines of incidence and refraction. But this law may be demonstrated independently of attraction. Therefore, before adopting the hypothesis of Newton to the exclusion of all the others, it is necessary to examine how far the different conclusions resulting from it are confirmed by experiment. Such are the objects of the researches of which the memoir of MM. Arago and Petit gives the first part.

The whole action of the body on light is measured by the increment of the square of the velocity of the ray, and this increment may be denoted by the term *refractive power.* This quantity ought obviously to depend on the nature of the body. But in the same substance it ought to remain proportional to the density; and *it is natural to think that an attraction always acts proportionally to the mass, whatever be the function of the distance according to which it varies.* On this supposition the refracting power, that is to say, the ratio of the refractive power to the density, ought to depend upon the chemical constitution of the body, and remain constant when the density alone changes.

This consequence of the theory of refraction has never been verified, except in the gases. But their refractive power is very weak, the increase of velocity which they communicate to light is very small, and a very simple calculation will show that the expression of the refractive power deduced from the Newtonian theory is not the only one which in the gases remains proportional to the density; and that there exists an infinity of expressions which would all satisfy the same condition. Therefore, though the gases appear to have a refractive power independent of their density, we have no reason to conclude that solid and liquid bodies possess this property.

The authors thought that the best way of deciding this question completely was to compare the refractive power of different liquids with that of the vapours which these liquids form. In this case the change of density is very considerable, and one of the bodies at least preserves a strong action on light. They made choice of the liquids which furnish the most abundant vapour at the ordinary temperature of the atmosphere. They measured the refracting power of each of these liquids, and that of the vapours derived from them. By comparing these powers with the known densities of the liquids and vapours, it was easy to see whether in each of these bodies the refracting power was independent of the density.

The result of their experiments proves the contrary. They all agree to give for vapours a refracting power sensibly less than that of the liquids which have formed them. Thus the refracting power of liquid carburet of sulphur, referred to that of air, is a little greater than three; while that of the same substance in a state of vapour, referred likewise to air, does not exceed two.

If we compare this result with theory, we find ourselves obliged, according to the Newtonian hypothesis, to suppose, what is certainly a singular supposition, that *the attraction of the same body for light does not act in proportion to its density.* Unfortunately, the number of bodies on which we can operate with precision in the state of vapour is too small to enable us to conclude from these experiments any law relative to the variation which the change of density makes the affinity of the body for light undergo. The liquids tried by the authors were carburet of sulphur, sulphuric ether, and muriatic ether.

In want of this direct method, the authors were of opinion that this law might be deduced from a comparison of the refractive power of gases, and that of the solid or liquid bodies which they form by uniting. If in the gaseous combinations which preserve the gaseous state, the refracting power of the compound were, as has been hitherto believed, equal to the sum of the refracting powers, it would follow that the act of combination will not in the least modify the action of the body on light, from which we may conclude with probability that the refracting power of a solid or liquid compound does not differ from the sum of the refracting powers of its gaseous principles, but in proportion to the increase which these last receive by condensation.

However, as the law relative to the refracting force of the compound gases had been established only on a small number of experiments, it was necessary, in the first place, to be certain of the accuracy of that law. But the measures which the authors have made of the refraction of a great number of gases have shown them that this law does not always agree with the results of observation.

We see, then, that the refracting power of a body, far from being constant, as the Newtonian theory would seem to prove, *in the most natural hypothesis which could be made relative to refraction,* undergoes, on the contrary, variations either from the effect of changes of density, or from the state of combination in which the body is. To determine the influence of each of these causes in particular, it is necessary to measure with accuracy the refracting powers of a great number of substances, and those of the compounds which they form. The work undertaken with this view comprehends already a considerable number of bodies. But the authors have felt the necessity of extending it still further before endeavouring to connect by any general law the different results which they have obtained.

To those who may start some doubts about what they consider as the *most natural hypothesis which can be made respecting attraction,* the authors oppose two passages, in which the author of the Mechanique Celeste has shown himself of the same opinion, expressing himself thus, book x. p. 274:—" I shall suppose that the value $\frac{K}{h\,z}$ is in the state of liquidity and of vapour. It is, in fact, the most natural hypothesis which we can admit." And in p. 23 of the preface to vol. iv.:—" I endeavour to supply it by supposing that the refracting forces of water and vapour are proportional to their respective densities; in this probable hypothesis," &c. But what appears at one time the most natural and probable, may cease to seem so when new facts have brought new light. And we may remark the reserve of the author of the Mechanique Celeste, who speaks of his calculation only as an essay which leaves him at liberty to try other means, if what presented itself at first was attacked by some fact or experiment which obliged him to reject it.

The facts established by MM. Arago and Petit appeared to them of such importance that they have been anxious to follow the consequences in the different phenomena which have a more or less direct connexion with that of refraction.

The coloured rays of which white light is composed are unequally separated from each other by their refraction in bodies of different natures, and this is what constitutes the difference in the dispersive force of bodies. It is most natural to measure the dispersive power by the different refractive power relative to the extreme colours of the spectrum; and in the theory of Newton, this difference ought to be constant for the same body, as well as the refracting power of the mean rays.

Experience having shown that this last power diminishes with the intensity, it was easy to foresee that the dispersive power would diminish also. But it was important to examine if these variations would follow the same law. To determine the point, it was necessary to ascertain the dispersive power of the liquids and vapours, whose refracting power had been determined by the preceding experiments. The dispersive force of the liquids was easily examined; but this was not the case with the vapours. The refraction which they occasion in a prism being very small, the dispersion, which is only a very small part of the refraction, is *scarcely sensible.* Accordingly, notwithstanding the importance of such a determination in gases and vapours, philosophers seem to have despaired of deducing them from observation. The object which the authors had in view required a direct measurement, and they have accomplished it by a method which they promise to describe in detail; and they announce that experiments made on the same vapour under different circumstances agree sufficiently with each other to show that their determinations approach pretty near the truth.

They have ascertained that the dispersive power really diminishes with the density; but that the dispersive power diminishes at a greater rate than the refracting power; so that if we call i the ratio of the sine of incidence to the sine of refraction, and ϱ the density of the body, the refractive power $\left(\frac{i^2 - 1}{\varrho}\right)$ is not only variable for the same class of rays, but the law according to which this change takes place is different for the different coloured rays.

In carburet of sulphur, already chosen as an example, the ratio of the dispersive power to the refracting power is 0·14 in a liquid state, while it is reduced to less than 0·08 in a state of vapour.

Thus while the variation of the refractive power may be still explained by admitting that the attraction of the same body for light varies according to a different law than that of the direct ratio of the densities, we see that, to explain the variation observed in the dispersive power, it would be necessary to suppose besides that the action of a body on the differently coloured rays follows, in the changes of density, a different law for each of these rays.

These different suppositions doubtless diminish both the simplicity and probability of the Newtonian theory. But before coming to any decision, the authors repeat that it is necessary to examine with a great deal of care the changes which the refracting forces of bodies undergo, either by variations of density, or by the effect of combination. It is no less indispensable to join to these determinations those relative to the dispersive forces, which philosophers hitherto have not examined, and which may by means of numerous precautions be deduced from direct experiments.

The work which the authors propose to publish on this subject is far advanced. They thought, however, that it might be useful to make known at present the results which they have drawn from their experiments on liquids and vapours.

Mecanique Analytique, by J. L. Lagrange; new edition, revised and corrected by the author. Vol. II. Paris. Mad. V. Courcier.

The editors, in a very short advertisement, give an account of the causes of delay in the publication of this second volume. M.

Lagrange had printed the first sheets when death snatched him from the sciences. M. de Prony undertook to complete the edition; and he was assisted in revising the proof sheets by M. Garnier, Professor at the Royal Military School. The manuscript of sections seventh and eighth was found in great order. That of section ninth was very incomplete, the first paragraph alone being finished. MM. Prony, Lacroix, and J. Binet, examined his manuscripts with the greatest attention, and were convinced that the author had proceeded no further, and that of course nothing had been lost.

M. Binet undertook the disagreeable labour of comparing the subjects and the notation of the old edition with what was printed of the new. Advantage was taken of all the marginal observations found in Lagrange's copy written with his own hand. Some things relative to rotatory motion, too incomplete to form a paragraph, have been thrown into a note at the end of the volume.

Another note has been formed of a remark likewise found among the manuscripts. It relates to the problem of determining the orbits of comets, a problem treated of in the third paragraph of section seventh.

The volume is terminated by a complete list of the works of M. Lagrange, communicated by M. Lacroix. At the end of this list we see with pleasure that a Minister, a companion and great admirer of M. Lagrange, caused Government, during his administration, to purchase all the manuscripts left by this illustrious mathematician; and at his request the Class of Sciences has named a commission to make choice of those that are fit for printing. The others will be classed, and placed in the library of the Institute.

(*To be continued.*)

Article XIV.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Mr. Clarke commenced his Course of Lectures on Midwifery, and the Diseases of Women and Children, on Monday, Jan. 27. The lectures are read every morning, from a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals, at the lecture room, No. 10, Saville-row, Burlington Gardens.

Middlesex Hospital.—Dr. Merriman and Dr. Ley will recommence their Lectures on the Theory and Practice of Midwifery, and the Diseases of Women and Children, at the above Hospital, on Monday, Feb. 17, at half past ten o'clock.

II. *New Swedish Minerals.*

Professor Berzelius was employed during last summer along with Assessor Gahn in examining the minerals in the neighbourhood of Fahlun. The mine of Finbo is in a granite vein which traverses gneiss. At the place where the pyrophysalites and yttroceratites have been found, this vein is more than 12 feet wide. But they have not yet been able to trace out its length. During their examination of this vein, they discovered several new minerals, of which the following are the most remarkable.

1. Orthite, so named because it always forms straight radii. It resembles gadolinite, but differs in its fusibility. It is composed of

Silica	32·00
Lime	7·84
Alumina	14·80
Protoxide of cerium	19·50
Protoxide of iron	12·44
Protoxide of manganese	3·44
Yttria	3·44
Water	5·36
	98·82

2. Neutral fluate of cerium, crystallized in regular six-sided prisms. It is composed of

Fluate of protoxide of cerium	30·43
Fluate of peroxide of cerium	68·00
	98·43

with some traces of fluate of yttria.

3. Subfluate of cerium. In it the fluoric acid is combined with twice as much of these bases as in the preceding mineral. It has a strong resemblance to porcelain jasper. Its colour is yellow, and its form gives marks of crystallization.

4. Fluate of yttria. It contains a good deal of silica. But Berzelius is not yet certain whether or not it be a fluosilicate.

The mine of Finbo yielded likewise a quantity of red opake emeralds, yttro-tantalites, and zircons. The fluates above enumerated are very rare. Only four or five pieces of the subfluate of cerium were found.

At Kararfvet, situated on the other side of Fahlun, there is another vein of granite, which contains crystallized gadolinites, some yttro-tantalites, and a variety of orthite, which has the curious property of taking fire before the flame of the blow-pipe, and of continuing to burn for some moments. It has received the name of *pyrorthite*. Besides the same constituents as the other orthite, it contains 25 per cent. of carbon. The gadolinite in this vein likewise contains carbon; but the quantity hardly amounts to half a per cent.

A full account of these minerals will be published in the fifth volume of the Afhandlingar by Berzelius.

III. *Measurement of the Earth.*

Several degrees of latitude are to be measured in Jutland by order of the King of Denmark. The operation will be conducted by Professor Schumacher, who has succeeded the late Mr. Bügge as Astronomer Royal.

IV. *London Charity Schools.*

From the returns made to the circular letters of the Committee of the House of Commons on the Education of the Lower Orders, printed in the Appendix to their Report, p. 556, it appears that the money annually spent in London on charity schools (not reckoning the Charter House, St. Paul's, Westminster, and many others) amounts to 41,089*l.* 3*s.*

The income of the Charter House is 22,384*l.* 10*s.* 5*d.*—See Second Report, p. 289.

V. *Heights near London.*

	Feet.
Thames at Hampton above the sea	14½
Low water at spring tides at Isleworth	1
St. Paul's church-yard, north side, and iron gallery over the dome	281
Top of St. Paul's stairs and said gallery	324
Top of Scotland Yard wharf, and the dining room of the Spaniard on Hampstead Heath	422
Great Pulteney-street, and the said dining room	352
Pagoda in Kew Gardens	116½
Gun Wharf in Woolwich Warren, and uppermost story of Shooter's Hill Inn	444
Bushey Heath, top of Stanmore Hill, Middlesex, above low water mark at Somerset House	478

The first eight of these heights were determined by Gen. Roy. (See Phil. Trans. 1777, p. 653. The last was determined by Col. Beaufoy.

VI. *Caldbeck Fells.*

Mr. Borie last spring examined the Caldbeck Fells in Cumberland, which he found to be principally composed of granite. In some places he observed the granite traversed by veins of quartz, some of them six feet wide, and running N. and S. The quartz in some veins is beautifully crystallized; in others, it is intermixed with mica and wolfram. One vein attracted his particular attention. It is five feet wide, runs N.N.W. and S.S.E., and quartz, which is the predominating ingredient, is associated with crystals of mica, molybdena, and crystals of asparagus-stone.

VII. *Improvement in the Oxygen and Hydrogen Blow-pipe.* By Dr. Clarke.

(To Dr. Thomson.)

DEAR SIR,

It may concern your chemical readers to be informed that, by an improvement which I have made in the mode of using the gaseous blow-pipe, for burning compressed *hydrogen* and *oxygen,* I have been able to add greatly to its power of fusion; and have removed an obstacle which has occasioned failure, in some instances, for the reduction of the *earths* to the *metallic* state.

Instead of using *water,* in the pneumatic cylinder adapted to the instrument by Professor Cumming, I have used *oil*; pouring in barely a sufficient quantity of sallad oil to cover the wire-gauze. It must have been obvious that the *water* was calculated to prevent the possibility of reviving the *metals* of the *earths,* in all instances where it was forced out of the jet, and came into contact with the fusing mass. The *oil* on the contrary rather tends to aid the experiment: it moreover sustains a more tranquil ebullition during the passage of the gas. The explosions which take place occasionally in the cylinder do not communicate combustion either to the *oil,* or to the *gas* within the reservoir. I caused three explosions, purposely, by suffering the gas to burn out; but not a drop of the oil was driven out, and the consequences were only very slight detonations within the cylinder. The other part of the improvement consists in substituting for the brass tube a thermometer tube with a very large diameter; in the use of which there is no danger; but the volume of the flame is such that I have fused 100 grains of platinum into a single brilliant globule upon charcoal; and in your next number I will point out a method of extending the use of this apparatus to the arts and manufactures. The combustion of iron, with it, affords one of the most splendid and striking experiments that can be conceived; causing a shower of fire.

Cambridge, Jan. 12, 1817. EDWARD DANIEL CLARKE.

VIII. *Height of Table Mountain.*

The altitude of Table Mountain, at the Cape of Good Hope, above the level of the sea, is 1087 yards.

IX. *Benzoic Acid as a Re-agent for Iron.*

Mr. Peschier, a skilful chemist and practitioner of pharmacy at Geneva, has found that the benzoic acid, and still better the alkaline benzoates, are very good and useful tests of the presence and quantity of iron contained in any solution. They precipitate iron readily and entirely, and, being cheaper and more easily obtained than the succinates, which are commonly employed for that purpose, are considered by Mr. Peschier as deserving the preference in chemical analysis. Another very valuable property of benzoic acid is, that neither the acid nor its salts exert any action upon manganese.

* * *

I think it necessary to state, by way of appendix to the above notice, that Berzelius, as long ago as the year 1806, in his paper on Sebacic Acid, published in the Afhandlingar, vol. i. p. 171, and translated into Gehlen's Journal (second series), vol. ii. p. 275, proposed benzoic acid as a good re-agent for separating iron from other bodies. Mr. Hisinger, in consequence of this proposal, made a set of experiments in 1810 on benzoate of ammonia as a re-agent. The result was a conviction that it would answer very well as a substitute for succinate of ammonia. This paper was published in the Afhandlingar, vol. iii. p. 152; and a translation of it appeared in the Philosophical Magazine, vol. xl. p. 258.—T.

X. *Queries respecting the Trigonometrical Survey.*

(To Dr. Thomson.)

SIR,

I should feel obliged if you would allow me to propose two plain and simple queries to the gentleman who signed himself R. M. A. in your last number. 1. Did the determination of the length of the pendulum, as connected with the subject of weights and measures, originally form one of the objects of the trigonometrical survey? 2. Did not the operations which are now pursuing on that important subject originate from the following address being moved in the House of Commons, by an Hon. Member of that House, on the 15th of March last?

Your humble servant,

Portsmouth, Dec. 7, 1816. CIVIS.

"That an humble address be presented to his Royal Highness the Prince Regent, praying that his Royal Highness will be graciously pleased to give directions for ascertaining the length of the pendulum vibrating seconds of time in the latitude of London, as compared with the standard measure in the possession of this House; and for determining the variations in the length of the said pendulum at the principal stations of the trigonometrical survey extended through Great Britain: and also for comparing the said standard measure with the ten millionth part of the quadrant of the meridian now used as the basis of linear measure on the continent of Europe." Which passed without opposition.

XI. *Some additional Particulars respecting the Earthquake in Scotland.*

(To Dr. Thomson.)

SIR, *Relugas, Nov.* 12, 1816.

In addition to the particulars of the earthquake of Aug. 13 last, published in your 47th number, I beg leave to send you the following facts lately come to my knowledge. The wall of a farm house in the immediate neighbourhood of Inverness was widely rent from top to bottom by the shock, in which situation it now stands. The people on board the dredging barge, moored at the foot of Loch Ness, although sensible of no motion in the water, were awakened and much alarmed by the *rombo*, thinking that the vessel had broke from her mooring chains; and the ferrymen, who happened to be on the ferry of Kessock at the time of the commotion, distinctly felt their boat heaved suddenly and rapidly, as if projected over two or three large waves; the night, and the general surface of the sea, being in other respects perfectly calm.

I am, Sir, your obedient humble servant,

Thomas Lauder Dick.

Errata in No. 47 of the Annals.

P. 347, line 21, *for* considerations, *read* consideration.
— 372, — 34, — rhombo, *read* rombo.
— 375, — 10, — waters, *read* water.

XII. *Models of Crystals to accompany Jameson's Mineralogy.*

My mineralogical readers will be gratified to be informed that the models of crystals to accompany Mr. Jameson's Mineralogy are now ready for sale, and may be had by applying to the maker, at Gee-street, Somer's Town.

XIII. *Discovery of the Yenite in Situ.*

Mineralogists will be happy to perceive that this hitherto scarce mineral is likely to become abundant, by the following extract of a letter, for which I am indebted to Mr. Mawe, mineral dealer, in the Strand:—

Rome, Oct. 26.

On a visit to the Isle of Elba, after much labour and research, I found the place where Le Lievre, Councillor of the Board of Mines, &c. discovered the yenite, when employed by his Government to inspect the mines of that island.

It was so concealed as to render it improbable that it should again be brought into notice. I rejoice at this event, as it breaks the monopoly, and will enable me to send specimens, for the advancement of science, to be sold by a respectable dealer in your metropolis, with a view to enrich my private collection by exchange, if possible.

Your obedient servant,

D. D. D.

XIV. *Mineralogical Examination of India.*

It must be rather mortifying to mineralogists that the peninsula of India, which has supplied the world for so long a period with some of the finest productions of the mineral kingdom, and which may now in some measure be considered as belonging to the British empire, should in a mineralogical point of view be still almost unknown. There is every reason to expect that this defect will now be remedied. Sir John Malcolm has taken with him to India Mr. Laidlaw, a gentleman educated as a civil engineer, and an excellent practical mineralogist and geologist, with the avowed intention of examining the country. We may anticipate from the labours of this gentleman numerous discoveries, which cannot but prove interesting to the scientific world, and of great importance to our Indian empire, from the new sources of wealth which they may disclose.

XV. *Queries respecting a Mode of stopping Fermentation.*

(To Dr. Thomson.)

SIR, *Bath*, *Jan.* 4, 1816.

I have been very frequently applied to by some friends (in the cider counties) for a method by which the fermentation of liquors may be stopped at pleasure. Being no chemist myself, I venture to address this question to you : should there be no such method at present known, it might be a subject worthy the attention of some of your chemical correspondents. A discovery of this nature would (if not too expensive) be of infinite value in the cider counties, as thousands of hogsheads might be saved, which are now annually spoiled by the fermentation proceeding too far. I believe one of the most common expedients now in use for checking it, or, to use the country phrase, for *preserving the sweets*, is to rack it repeatedly from one cask to another, and to suspend a lighted rag, previously dipped in sulphur, in a barrel half full of the cider, and by means of agitation to impregnate the liquor with the smoke arising from it; but this method is at least very uncertain and imperfect, and requires more attention (particularly in the racking) than can generally be spared to it.

I have long wished to call the attention of some of your correspondents to this subject, but have hitherto been deterred, from the fear of troubling you with what, perhaps, might be beneath your notice. I have, however, ventured to write this; and can only say that, should I be thought troublesome in so doing, I must plead ignorance, and a wish to do good to a numerous class of individuals, as my excuse.

I beg also you will do me the honour to insert this letter either in your *Annals*, or into your fire, as may appear to you to be its most proper destination.

I have the honour to remain,
Sir, your most obedient humble servant,
E. S. STRANGWAYES.

XVI. *Queries respecting New Holland.*

(To Dr. Thomson.)

SIR,

In consequence of reading in the Times newspaper an account of the further progress of the discovery of the interior of New Holland, I referred back again to your last January number (1816), in which the particulars of Governor Macquarrie's expedition are detailed; and in p. 77 I observe he says, "The Governor must, however, add, that the hopes which were once so sanguinely entertained of this river becoming navigable to the Western Sea have ended in disappointment." I should like to learn what were the reasons or the facts which led him to this conclusion, and it is somewhat singular he does not state either the size or the nature of this river. In the account published in the Times it is stated that Mr. Evans, in his last excursion, fell in with a large river, which he conceives would become navigable for boats at the distance of a few days' travelling, and he conjectures it must join the Macquarrie river.

Perhaps, through the medium of your publication, some light may be thrown on these very interesting questions, connected with the geography of this singular continent, especially in reference to the curious account given by Flinders of the steep rocky bank of the Southern Coast, which it is most singular he never seems to have thought of endeavouring to ascend.

Your obedient servant,

L. J.

All the accounts of Mr. Evans's journey which I have seen convey very little information, because we are not told the direction in which he travelled. The *navigable river*, as far as I can make it out, is merely that he fell in with a river which, if he had traced it far enough, he had no doubt *would have become navigable.*—T.

XVII. *Existence of a Stone in a Coal Bed.*

(To Dr. Thomson.)

DEAR SIR,

The following circumstance, which is considered curious, happened in a coal-mine at Cockfield, in the county of Durham, four years ago:—

A man hewing coal struck upon a substance which his pick would not enter. He immediately took down his lamp, for the purpose of examining what it was that he was unable to penetrate; whereon he discovered a large piece of stone. The stone was soon got out; and as soon as this was accomplished, he ascended to show it to the banksman, who was very much surprised, having never before seen such a thing It was next shown to Mr. D., the proprietor of the colliery, who examined it carefully, and found it to be flint. Mr. D. was kind enough to show it to me; but I certainly do not agree with him as to its being flint. The colour is bluish-grey, with a streak of blue silver purple running through it. Mr. D. informed me that, although he had been the owner of three different coal-mines upwards of half a century, an instance of this nature had never before occurred. This, although a trifling communication, may probably prove worthy of a place in your Journal. The account is given nearly verbatim as I had it from Mr. D.

I remain, dear Sir, yours sincerely,

Dec. 24, 1816. R. S. M. R. M. S. E.

XVIII. *Proposed Improvement in Brook's Blow-pipe.*

(To Dr. Thomson.)

SIR,

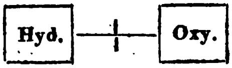
Considering what advantages may accrue to chemistry from Newman's blow-pipe, if constructed upon a safer principle, I have been induced, though young and inexperienced, to give you my ideas upon the subject.

What I have to propose is, a small alteration on the improvement of Mr. Edwards, as given in Burrows's Medical Repository for November last. Instead of dividing the reservoir in the middle by a plate, I would have the gases quite separated in two distinct reservoirs; for I conceive, if any accident should occur to the centre plate, so as to permit the gases to mix, an explosion would ensue, and perhaps be the more dreadful because the less expected.

If it should be found that, according to Mr. Edwards' plan, the gases do not properly unite, by merely coming in contact as they issue from the extremity of the cap, it would probably be attended with very little danger, if an explosion *should* happen, if they were to be mixed in a small quantity *in* the cap, to which a capillary tube might be adapted, like the one in Brande's Journal, No. III.

As the reservoir for oxygen need not be so large as the one for hydrogen, the former might be (when not in use) put into the latter, if the latter could be so constructed without allowing the escape of the gas, which would render it more portable than any yet proposed.

The two reservoirs, for the sake of firmness, might be easily connected (when in use) by a bar of metal screwed into their sides, with another piece through it for a handle.



I merely send this as a hint for your superior judgment to dilate upon, trusting that my youth will be a sufficient excuse for all mistakes, and that my boldness may be attributed to my love of chemistry.

Dec. 23, 1816. B. P.

XIX. *Query respecting the Combustibility of Clay.*

(To Dr. Thomson.)

SIR,

The Lord Mayor having proposed a method of burning clay and mud, under the impression, as I suppose, of the caloric being retained a little longer by its admixture with the coal: but as clay will still be clay, whether combined or not, I shall be particularly obliged if you will inform me, by means of your Journal, *if it can be rendered combustible, or if it can only be ignited.*

I remain, yours respectfully,

Dec. 16, 1816. L. M. N.

P. S. If it can be rendered combustible, you will perhaps notice the quantity of the ingredients.

It is so universally known that clay is not a combustible substance, that I can scarcely bring myself to believe that my correspondent is serious in the question which he has proposed. I have often seen the method suggested by the Lord Mayor practised in some parts of Scotland, which happen to be at a distance from coals, and far removed from water carriage. There can be no doubt that the dross of coals, which are destitute of the property of caking, may be rendered capable of combustion by this means, which it can scarcely be said to be while in powder. How far this mode of manufacturing the dross of caking coals would be an improvement, is a different question.—T.

XX. *Meteorological Register at New Malton, Yorkshire, for October, November, and December,* 1816. By Mr. Stockton.

October.—Mean of barometer, 29·65; max. 30·09; min. 28·98; range, 1·11 inch. Spaces described, 5·94; number of changes, 17. Mean of thermometer, 49·27°; max. 65°; min. 32°; range 33°. Mean of de Luc's whalebone hygrometer since the 19th, 72·5°.—Prevailing winds, N. and E. N., 1; E., 2; N.E., 7; S.E., 3; S., 5; W., 4; S.W., 3; N.W., 4; Var., 2. — Rain, 3·02 inches; total, 5·50 inches; wet days, 6; stormy, 2.

During the nights of the 1st and 2d of this month the wind blew briskly from the S.W.; but soon after sun-rise it veered to the opposite quarter, and was succeeded by heavy and incessant rain. When the moon attained her full, on the 6th, there was a considerable increase, both of pressure and temperature; and although we had a steady heavy rain from the N. E. on the 8th, yet the barometer kept steadily rising. The weather now became quite mild and autumnal, and the pressure and temperature continued high, and pretty uniform, until the last quarter, when both sustained a rapid depression. The winds, on the 19th and 20th, were very high from the W. and N.W., and particularly by night. On the 21st the air, which had long been cloudy, suddenly cleared up, and the next morning the temperature continued until nine at the freezing point. The frost this morning was extremely keen; but the wind veering from N.W. by W. to the S.W., with aboundance of the *Cirrostratus,* and the hygrometer at 86, indicated a change, which very speedily took place; for the 24th and 25th were exceedingly wet, with high winds; and the loss in the barometrical column in about 24 hours was nearly equal to an inch of quicksilver. The temperature, from this time to the close, with a single exception, was uniform, varying little by day or night, and a very gradual decrease of pressure from the first quarter of the new moon was followed by a heavy fall of rain.

November.—Mean pressure of barometer, 29·562; max. 30·76; min. 28·35; range, 2·41 inches. Mean temperature, 37·480°; max. 50°; min. 23°; range, 27°. Mean of hygrometer at nine, a.m. 70½ nearly; snow and rain, 3·02 inches; wet days, 14; stormy, 6.—Prevailing wind, southerly. N., 2; E., 1; N.E. 2; S.E., 1; Var., 2; S., 9; W., 5; S.W., 5; N.W., 3.

The changes in the pressure and temperature of the atmosphere during the former part of this period were so rapid and considerable, particularly in the former, as to be almost without precedent. The gradual depression of the barometrical column which marked the close of the last month continued with much rain until the 4th, when a brisk wind from the N.E. caused it to rise nearly half an inch. The next day, when the moon attained her full, a very sudden depression of eight tenths took place in a few hours, and was followed by a heavy storm of wind and rain from the north. The 8th was frosty and clear until sun-set, with the column steady, but very soon afterwards the clouds assumed their wintry appearance, and much snow fell, with the wind at S.W. until ten, when, it having veered to the southward, the weather became most tempestuous, with continued heavy rain, until seven the next morning. The barometer during the night fell from 29·35 to 28·45; but the air now clearing up, and the wind N.W., this loss was regained in about six hours. The 10th, like the 8th, was clear and frosty until three, p. m. when abundance of linear *Cirri* appeared, and began to extend in all directions, followed by the *Cirrostratus*. The wind went from N.W. by W. to the S., and (if possible) a more tempestuous night than that of the 8th was experienced, with much snow and rain. The loss of quicksilver during the day, from eight, a. m. to eleven, p. m. was equal to a full inch; but the storm having then a little abated in its violence, this quantity (with an increase of nearly half an inch) was almost as rapidly restored. Snow fell daily from this time to the 18th, attended on the 14th with a violent gale from the S., and incessant vivid lightning and loud thunder for nearly three hours in the S., W., and S.W. The weather on the 20th became more settled; and continued calm and frosty, with little variation, to the close of the month. On the last day the barometer, which for six preceding days had gradually been rising, remained stationary at 30·76, the maximum of the period.

December.—Mean pressure of barometer, 29·495: max. 30·76; min. 27·90! range, 2·86 inches. Spaces described, 15·87 inches. Number of changes, 23. Mean temperature, 34·382°; max. 48°; min. 16°; range, 32°. Mean of de Luc's hygrometer at nine, a. m. 76¼.—Prevailing winds, W. and S.W. N., 3; S. E., 1;

S., 8; W., 4; S.W., 11; N.W., 1; Var., 3. Snow and rain, 3·02 inches, which exactly agrees with the amount for the last month.

The changes in the density of the atmosphere again form the most prominent feature in the register; and the ranges in the short periods about to be noticed are most extraordinary. On the 1st the column indicated the maximum which closed the preceding month; but from the afternoon of this day to the 6th there was a very gradual depression of about two tenths of an inch daily; and during the night, which was stormy, with snow, the loss of quicksilver was equal to a full inch. On the following day this quantity was nearly restored. Rain by day, and frost and snow by night, continued, with an unsettled and depressed barometer, and high winds, until the 14th; when the wind at midnight became most tempestuous, with heavy rain and snow. Of the violence of this storm here (and it appears to have been general) some idea may be formed from its effect on the barometer, which from ten, p. m. (the 13th) to eight, a. m. the next day, was depressed nearly an inch and a half, viz. from 29·30 to 27·90! At this unprecedented point of depression it remained stationary above eight hours, with the wind at S.; nor did the column begin to rise until the current became more westerly; and an increase of seven tenths on the 15th was succeeded by a similar decrease on the 17th. Notwithstanding the wind blew furiously from the N. and N.W. during the night of the 18th, with abundance of snow, another increase of a full inch took place; and the barometer kept steadily rising the whole of the next day, which was also stormy, with heavy showers of snow, when it nearly indicated its former maximum of elevation. A severe frost, with a dense and clear atmosphere, marked the 20th; the temperature at eight, a. m. being 16° below the freezing point. On the 21st the diurnal temperature never passed 23°; but about ten, p. m. it had risen to 27°; and the wind changing from N. to S.W., a thaw immediately followed, with rain. A very sudden increase of temperature, an unsettled barometer, and violent gales, accompanied with driving showers, continued to the 28th, when the wind blew violently from the S., with heavy rain. The barometer during the night again parted with another inch of quicksilver; and the next morning, the wind having veered to the N.W., this loss was regained.

James Stockton.

XXI. *Philosophical Apparatus.*

Mr. Singer is prevented from continuing his public lectures by severe indisposition, which renders it uncertain at what time they may be resumed. The extensive collection of philosophical instruments hitherto employed in illustration of these lectures are in consequence to be brought to public sale in a very short time.

XXII. *Meteorological Table. Extracted from the Register kept at Kinfauns Castle, N. Britain. Supposed Lat.* 56° 23½′. *Above the Sea* 129 *feet.*

1816.	Morning, 8 o'clock. *Mean height of* Barom.	Morning, 8 o'clock. *Mean height of* Ther.	Even., 10 o'clock. *Mean height of* Barom.	Even., 10 o'clock. *Mean height of* Ther.	Mean by Six's Ther.	Depth of Rain. In. 100	No. of days. Rain or Snow.	No. of days. Fair
Jan.	29·424	34·677	29·435	34·580	35·45	2·30	13	18
Feb.	29·651	33·724	29·683	34·000	34·58	0·80	7	22
March	29·642	35·838	29·655	35·612	37·35	1·60	13	18
April........	29·714	39·933	29·697	38·766	41·30	1·00	8	22
May	29·762	47·129	29·768	45·322	48·64	2·30	10	21
June	29·812	53·400	29·821	50·266	54·46	1·30	8	22
July	29·560	55·710	29·550	53·330	56·29	4·25	16	15
Aug.	29·804	54·550	29·807	53·070	56·19	3·15	10	21
Sept.........	29·729	50·060	29·721	48·900	51·20	2·75	16	14
Oct.	29·726	44·710	29·710	46·320	46·58	2·15	10	21
Nov..........	29·615	38·466	29·622	38·333	39·26	1·65	8	22
Dec.	29·494	33·871	29·489	33·806	34·51	1·70	13	18
Aver. of year.	29·661	43·505	29·663	42·692	44·65	24·95	132	234

ANNUAL RESULTS.

MORNING.

Barometer. *Observations.*	*Wind.*		Thermometer.	*Wind.*	
Highest, Nov. 30	W	30·67	July 21	S W	62°
Lowest, Jan. 17	W	28·40	Dec. 13	W	15°

EVENING.

Highest, Nov. 30	W	30·67	Sept. 14	S W	60°
Lowest, Jan. 12	S W	28·60	Jan. 29	N E	19°

Weather.	*Days.*
Fair	234
Rain or Snow................	132
	366

Wind.	*Times.*
N and N E	32
E and S E	105
S and S W	62
W and N W.................	167
	366

Extreme Cold and Heat, by Six's Thermometer.

Coldest, December 13, Wind W.........................	13°
Hottest, June 24, Wind E.............................	72
Mean temperature for 1816..........................	44° 65′

Result of three Rain Gages.	100 *In.*
No. 1. On a conical detached hill above the level of the sea 600 feet....	52·43
No. 2. Centre of the garden, 20 feet..............................	24·95
No. 3. Kinfauns Castle, 129 feet	19·61
Mean of the three gages..	32·33

Article XV.

New Patents.

John Burnett, of Bristol, iron-founder; for his convolving iron axletree for the reduction of friction and animal labour, by the application of which wheels of carriages of every description are prevented from coming off whilst travelling, and carriages are drawn with less animal labour. June 20, 1816.

John Hawkins Barlow, of Leicester-place, Leicester-square, goldsmith and jeweller; for certain improvements on tea-urns, tea-pots, tea-boards, or tea-trays. June 27, 1816.

John Barlow, of Sheffield, founder; for a new cooking apparatus. July 2, 1816.

John Towers, of Little Warner-street, Cold Bath-fields, chemist; for a tincture for the cure and relief of coughs, asthmas, and diseases, which he intends to denominate Towers's New London Cough Tincture. July 11, 1816.

Henry Warburton, of Lower Cadogan-place, Chelsea, Esq.; for a method of distilling certain animal, vegetable, and mineral substances, and of manufacturing certain of the products thereof. July 27, 1816.

Article XVI.

Scientific Books in hand, or in the Press.

Mr. Andrew Horn, author of the "Seat of Vision determined by the Discovery of a new Function in the Organ," proposes to publish by subscription a work upon which he has been long engaged—Illustrations of the Mosaic Cosmogony and Naochian Deluge. It is divided into three parts, viz. An Inquiry into the Origin of the Notion prevalent among Mankind concerning superior Beings: on Philosophical Cosmology: and on the Origin of the World, Formation and Revolutions of the Earth according to the Principles of Moses; this part includes 15 chapters. The whole will compose one volume, 4to. of about 500 pages, accompanied with four plates, illustrative of the various subjects and theories which it embraces.

An Inquiry into the Effects of Spirituous Liquors on the Physical and Moral Faculties of Man, and on the Happiness of Society.

The Third Volume of the Zoological Miscellany is preparing for the Press, and will be published in the course of two or three months.

Mr. James White, author of the very popular work on Farriery, is preparing for publication a compendious Dictionary of the Veterinary Art.

Dr. Burrows is preparing Commentaries on Mental Derangement.

The Rev. Dr. Chalmers, of Glasgow, is printing a Volume of Discourses, in which he combats at some length the argument, derived from astronomy, against the truth of the Christian Revelation; and, in the prosecution of his reasoning, he attempts to elucidate the harmony that subsists between the doctrines of Scripture and the discoveries of modern science.

ARTICLE XVII.

METEOROLOGICAL TABLE.

1816.	Wind.	Barometer.			Thermometer.			Hygr. at 9 a. m.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
11th Mo.										
Nov. 12	N W	29·70	29·23	29·465	52	38	45·0	75	1	☾
13	W	29·68	29·64	29·660	56	39	47·5	63		
14	W	29·64	29·44	29·540	45	27	36·0	67		
15	N W	29·73	29·44	29·585	37	28	32·5	65		
16	N W	30·04	29·73	29·885	38	28	33·0	70		
17	S W	30·04	29·68	29·860	43	28	35·5	90		
18	S W	29·75	29·68	29·715	47	32	39·5	93	7	
19	S W	29·93	29·75	29·840	46	39	42·5	95		●
20	S	30·00	29·97	29·985	51	37	44·0	76		
21	S E	29·97	29·78	29·875	44	29	36·5	68		
22	E	29·78	29·75	29·765	38	26	32·0	67		
23	N E	29·89	29·74	29·815	33	17	25·0	82		
24	N W	29·93	29·89	29·910	30	18	24·0	90		
25	S E	29·93	29·88	29·905	40	25	32·5	98	—	
26	N	30·22	29·93	30·075	42	30	36·0	96	·41	☽
27	W	30·30	30·22	30·260	45	32	38·5	80		
28	N W	30·43	30·30	30·365	44	32	38·0	98		
29	N	30·56	30·43	30·495	44	30	37·0	77		
30	N	30·62	30·56	30·590	38	30	34·0	78		
12th Mo.										
Dec. 1	N W	30·62	30·40	30·510	36	25	30·5	73		
2	W	30·35	30·33	30·340	38	28	33·0	92		
3	E	30·35	30·32	30·335	40	32	36·0	91		
4	E	30·32	30·08	30·200	42	36	39·0	80		○
5	S E	29·52	29·45	29·485	43	36	39·5	68	·11	
6	S W	29·45	29·35	29·400	40	29	34·5	75	·21	
7	S W	29·52	29·47	29·495	39	27	33·0	95	·18	
8	S W	29·69	29·52	29·605	37	25	31·0	95	—	
9	W	29·55	29·46	29·505	42	27	34·5	88	·17	
10	Var.	29·25	29·20	29·225	46	35	40·5	77	·13	
11	N W	29·36	29·25	29·305	40	27	33·5	95		
		30·62	29·20	29·866	56	17	35·80	82	1·29	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Eleventh Month.—12. Windy. 13. Strong breeze: sunshine: much water out in the marshes. 14, 15. Breezy: sun and clouds. 16. Slight hoar frost. 17. *Cirrostrati* at a great elevation, in which a solar halo appeared for an hour or two, a. m. 18. Max. temp. at nine, a. m.: windy: overcast: dripping forenoon. 19. The diurnal temperature disturbed by the solar eclipse (of which see the particulars in vol. viii. p. 467). 20. Fair: at two p. m. *Cumulostrati* formed rapidly, and passed off at a great elevation, the wind veering S. 21. Fair: rather windy. 22, a. m. Cloudy: steady breeze: p. m. *Cumuli.* 23. Cloudy: steady breeze. 24. A serene sky. 25. Hoar frost, the third morning: overcast with *Cirrostratus* and haze: rain at night. 26, a. m. Very misty: *Cirrostratus* sweeps the ground: rain. 27. Much *Cirrostratus,* especially to the N., of delicate structure: fair day. 28. The sky was so completely shrouded in a *Cirrostratus,* without the smallest opening to admit the sun's rays, that from nine to three the temp. did not ascend 2°: at sun-set the sky cleared pretty suddenly, showing red *Cirri* above for a considerable time: the lower air, which had been transparent, now filled with mist. 29, a. m. Misty: calm: very bright sun at mid-day: lunar halo. 30. Hoar frost: breeze: very fine day.

Twelfth Month.—1. Cloudy morning. 2. Lightly clouded. 3. Misty by *Cirrostratus,* soon after sun-rise, during which a hoar frost formed: grey lofty sky. 4. Grey: little wind. 5. Idem.: the lunar eclipse not visible for clouds: much wind, with rain, after. 6. Fine. 7. Very fine day: but a stormy night. 8. Rain, the middle of the day: clear night. 9, a. m. Very white frost, and rime on the shrubs: *Cirrostratus* floating at an elevation of two or three yards: the temp. rose quickly, and it rained, p. m. and night. 10. Fine day, with *Cirrus* and *Cirrostratus:* wet and stormy fore part of night. 11. Fair: at sun-set a lofty and wide spread *Nimbus* in the N. W.: at ten p. m. a bright shooting star to the W.: some large flakes of moist snow in the night.

RESULTS.

Winds variable and moderate till after the full moon.

Barometer :	Greatest height	30·62 inches.
	Least	29·20
	Mean of the period	29·866
Thermometer :	Greatest height	56°
	Least	17
	Mean of the period	35·80
Mean of the Hygrometer		82°
Rain		1·29 inch.

The rain fell at three distinct intervals, and chiefly by night, increasing greatly each time in quantity and continuance.

Tottenham, *Twelfth Month*, 14, 1816. L. HOWARD.

METEOROLOGICAL TABLE.

1816.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Hygr. at 9 a. m.	Rain.	
12th Mo.										
Dec. 12	S W	29·00	28·65	28·875	47	32	39·5	70	·40	☾
13	N W	29·35	29·00	29·175	43	28	35·5	76	—	
14	S W	28·82	28·53	28·675	47	33	40·0	65	·52	
15	W	29·38	28·82	29·100	41	29	35·0	81		
16	Var.	29·49	29·31	29·400	38	32	30·0	97	—	
17	Var.	29·28	29·22	29·250	49	35	42·0	92	·20	
18	N	30·09	29·28	29·685	44	33	38·5	70	—	
19	N	30·47	30·09	30·280	36	25	30·5	74	—	●
20	N E	30·47	30·35	30·410	32	22	27·0	85		
21	N	30·35	30·07	30·210	28	14	21·0	75		
22	Var.	30·15	30·00	30·075	31	17	24·0	93		
23	S W	30·00	29·66	29·830	46	32	39·0	83	·10	
24		29·72	29·53	29·625	48	33	40·5			
25		29·72	29·38	29·550	48	35	41·5			
26		29·42	29·27	29·345	50	33	41·5	90	·37	☽
27	N W	29·78	29·42	29·600	43	27	35·0	90	—	
28	S W	29·40	29·30	29·350	49	32	40·5	64	·18	
29	W	29·91	29·40	29·655	42	34	38·0	83	—	
30	S E	29·67	29·62	29·645	44	37	40·5	99	1·08	
31	S W	29·67	29·51	29·590	48	39	43·5	88	9	
1817.										
1st Mo.										
Jan. 1	S	29·35	29·30	29·325	48	36	42·0	90	·35	
2	S W	29·49	29·45	29·470	44	32	38·0	94	·72	
3	S	29·63	29·20	29·415	48	31	39·5	87	—	○
4	W	29·73	29·12	29·425	52	36	44·0	72	·76	
5	W	29·73	29·52	29·625	44	32	38·0	80	·70	
6	S W	30·25	29·52	29·885	45	33	39·0	65	·14	
7	N W	30·42	30·25	30·335	38	22	30·0	90		
8	N	30·53	30·43	30·480	30	26	28·0	82		
9	E	30·58	30·53	30·555	30	21	25·5	93		
		30·58	28·53	29·649	52	14	36·10	83	5·61	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Twelfth Month.—12. A wet day after a frosty night: the fore part of this night a violent storm of wind from the westward, the barometer rising fast. 13. a.m. Calm, with a turbid sky: about noon a clap of thunder, followed by some heavy sweeping hail. 14. The day fine, with *Cirrus:* after dark, the sky being suddenly overcast, the wind rose to an excessive degree of violence, with rain: the barometer had fallen since noon rapidly, the minimum (which is also the lowest point for the year) occurred very early in the morning of 15. During the storm in the night I was twice sensible of a tremor of the earth, distinct from the effects of the wind, and lasting perhaps a quarter of a minute. This I found reason to attribute to the shock of electrical discharges, as I found it had thundered twice about the time. 15. a.m. A gale, with clouds: the day fine, and windy afterwards. 16. Hoar frost: fair, with *Cirrostratus:* at night a small meteor moving eastward. 17. a.m. Wet: the wind S.E. 18. The wind passed by W. to N., and gradually rose to a moderate gale: a few drops about noon. 19. Wind inclining to N.E. a stiff breeze: snow, p.m. part of which lay on the ground. 20. A brilliant evening twilight, which was reflected by haze in the eastern sky. 22. Clear, save a little *Cirrostratus:* wind gentle and variable. 23. a.m. Wind rising, the air turbid: sleet and rain followed, with a windy night. 25. Very fine day, the barometer nearly quiescent at 29·72 till evening: at night the wind rose, and was boisterous to 26, on which day much rain, in squalls, p.m.: a lunar corona at night. 27. *Nimbi:* the sun set fiery red, and much enlarged: windy. 28. Hoar frost: fair day: night very tempestuous, with rain from the southward, which began, with the rise of the barometer, at 10 p.m. 29. Wind, followed by *Cirrocumulus*, and a calm night. 30. A very wet day and night. 31. Misty: little wind.

1817. *First Month.*—1. Windy: wet, p.m. 2. Fair: at five, p.m. hygr. 65°, and the moon yellow: notwithstanding these indications, there fell much rain and snow after it in the night. 3. Fair day, save a slight shower: the night (after bright moonlight) very stormy, with rain. 4. Small driving rain: at night another gale of wind. 5, 6, 7. Much the same alternations as for several preceding days. 8. Very white rime: misty about noon: *Cirrostratus.* 9. The wind has now gained the E., having gradually shifted round by N.: hoar frost: misty air.

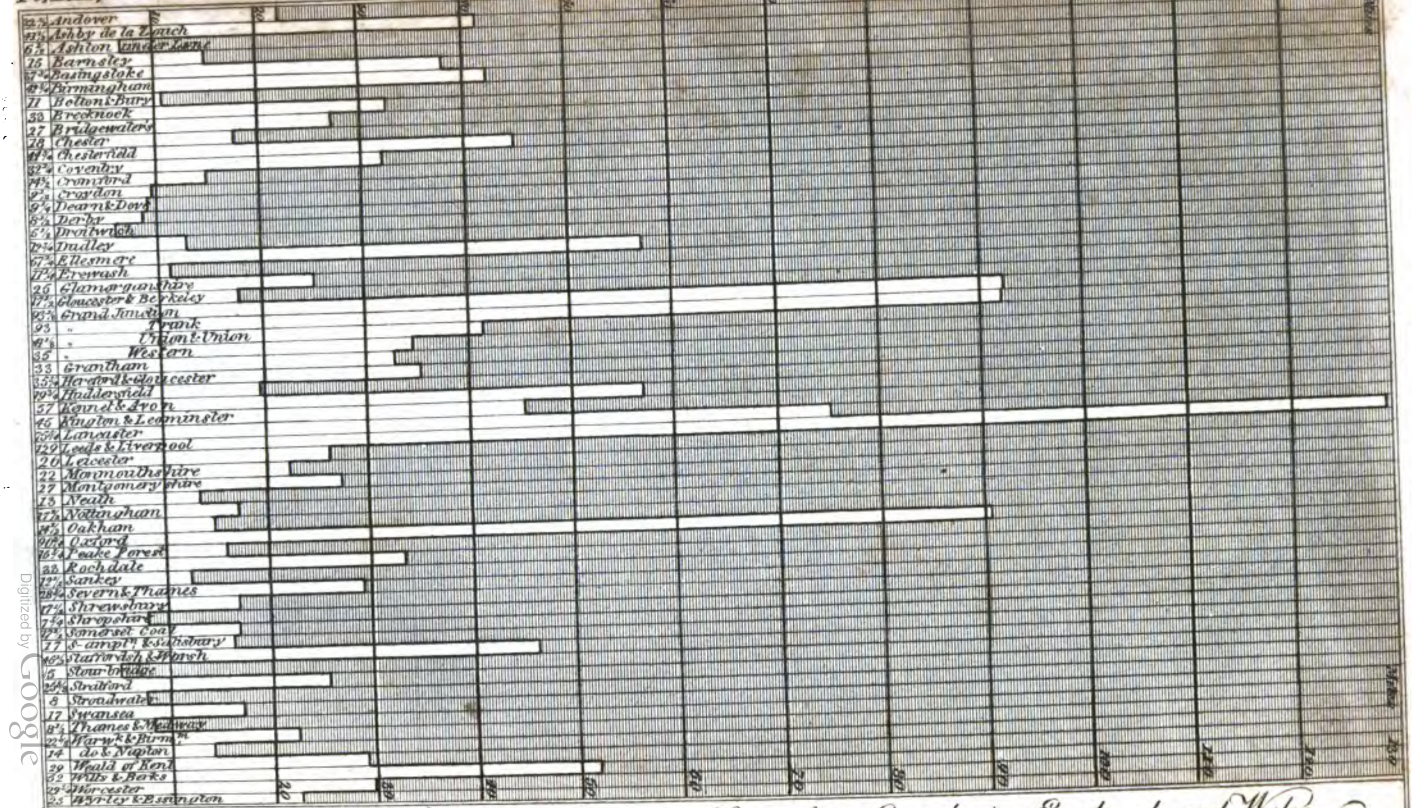
RESULTS.

Barometer:	Greatest height	30·58 inches;
	Least	28·53 inches;
	Mean of the period	29·649 inches.
Thermometer:	Greatest height	52°
	Least	14°
	Mean of the period	36·10°
Mean of the hygrometer		83°
Rain		5·61 inches.

The wind, though chiefly westerly, has been very variable in direction, and equally so in force; presenting a succession of heavy gales, with intervals of frost and rain. This enormous quantity of rain being *twice* as much as usually constitutes a *wet moon* in this part of the island, had the usual effect of inundating the country to a great extent, especially when met by the spring tide after the full. By a mark preserved at the Laboratory, however, I find that in the inundation of 1809 the river Lea rose 15 inches higher than on the present occasion.

Tottenham, *First Month*, 18, 1817. L. HOWARD.

Pl. LXII,



A Chart exhibiting the Length of the main Line of 60 Canals in England and Wales

ANNALS

OF

PHILOSOPHY.

MARCH, 1817.

ARTICLE I.

On Canal Levels. By Samuel Galton, Esq. F.R.S.

[With Three Plates.]

(To Dr. Thomson.)

SIR, *Birmingham, Dec.* 31, 1816.

IT occurred to me several years ago that the lockage of canals, and their plans and sections, would afford the means of ascertaining, with a considerable degree of comparative precision, the relative height or level of all the places immediately situated upon those canals which communicate with one another; and that, in consequence, a number of fixed points would be obtained, from which the relative level of any objects in the vicinity of those canals might be more conveniently measured.

Being possessed of several plans and surveys of canals, and having access to a still superior collection made by the direction of a canal committee, of which I am a member, I have made sections of most of the canals in the kingdom.

In connecting these sections, I observed, with some surprise, that the Thames at Brentford appeared to be nearly 14 feet lower than the junction of the Duke of Bridgewater's Canal with the Mersey, at Runcorn. Hence I concluded that there must be an error in the surveys, or that the levels of the German Ocean, and of the Irish Channel, are very different. In your *Annals* this fact is stated. I shall, therefore, with great pleasure, send to you some of the plans and sections which I have taken.

1. The first, which I inclose, is a synoptic view of the principal canals in England, in reference to their length only. (Pl. LXII.)

2. A synoptic view of the rise and fall of several canals in connexion, without any regard to their respective length, which the first chart will show. (Plate LXIII.)

3. A section of the canal communication from the Mersey at Runcorn, to the Thames at Brentford, showing both the length and the lockage. (Plate LXIV.)

In the second chart I have taken as zero the summit of the Birmingham Canal, which is at Smethwick, about three miles from Birmingham, and continues to Wolverhampton. This point is chosen on account of the central situation of the Birmingham Canal, its connexion with other canals, the height of the summit, and because the water from that summit falls, part of it into the Irish Channel, and part into the German Ocean.

The authority on which the rise and falls of the several canals are taken is stated, that any correction, if requisite, may be made with greater facility.

Amongst the documents which have been consulted are—Smeaton's Canal Reports; General History of Canals, by John Phillips, 1803; Carey's Navigable Canals of Great Britain; A. Smith's Map of Canals, 1815; Jos. Plymley's Agricultural Report of Shropshire, in which the article of Canals was furnished by Thomas Telford; Rees's Cyclopedia, article Canals, 1805; Sutcliffe on Canals, 1816; a great variety of surveys and plans of canals; private correspondence, and official information.

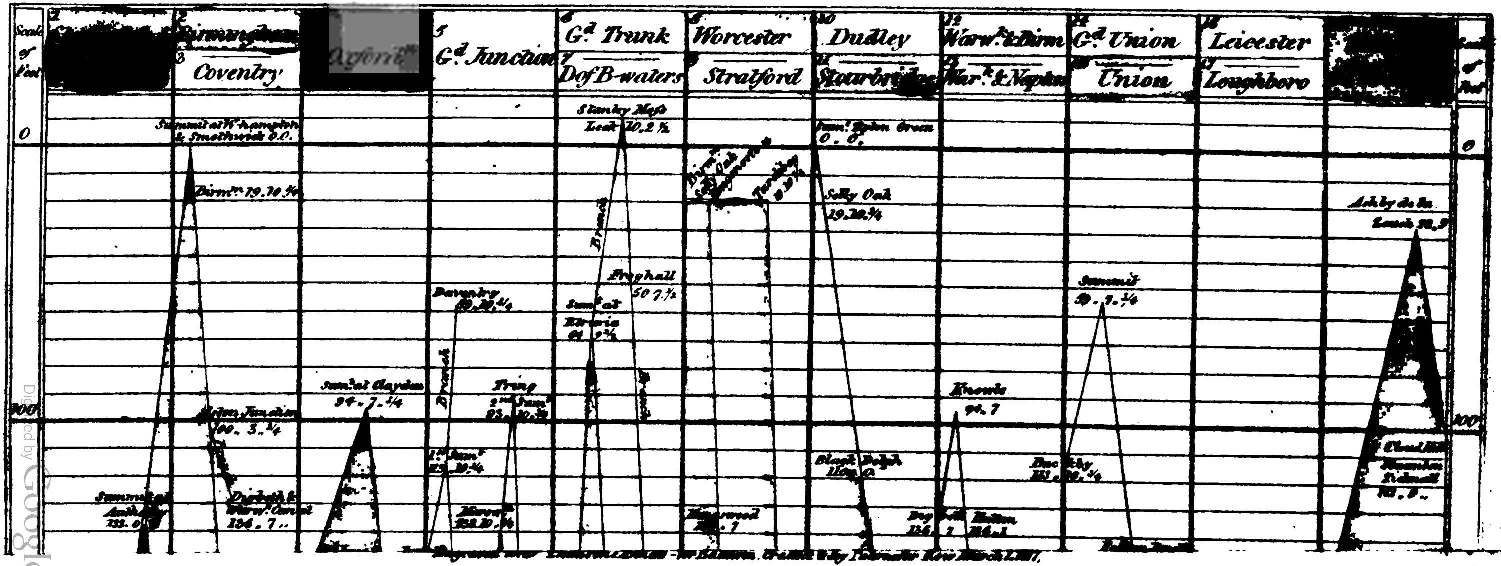
If observations were made with Sir Henry Englefield's barometer, at various situations, on different canals, on the same days, viz. on March 31, June 30, Sept. 30, and Dec. 31, and at the same time, would not they afford some confirmation of these surveys, or lead to some further investigation?

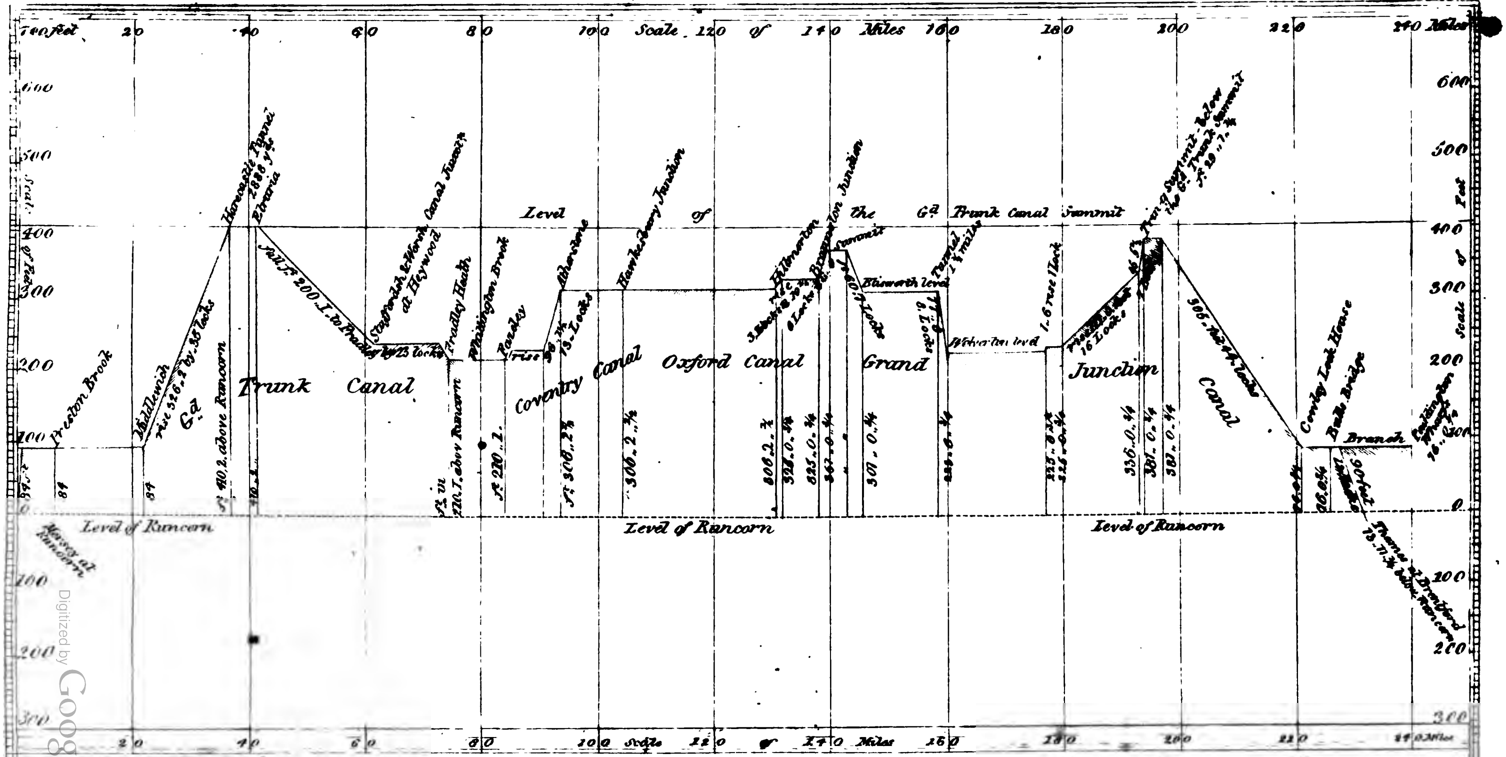
Information on the following heads is among the desiderata on this subject:—

1. The level of the *sill of the lock* of the Grand Junction Canal at Brentford (in reference to the summit at Tring), instead of the present reference to *high water mark* in the Thames.

2. The level of the sill of the lock of the Duke of Bridgewater's Canal at Runcorn, where it joins the Mersey, in reference to the sill of George's Dock at Liverpool; and the level of the sill of George's Dock, in reference to the surface of the water in the basin of the Leeds and Liverpool Canal at Liverpool. This would furnish a series of levels from London to the River Aire, at Leeds; and give fixed points more certain than any which refer to the tides; and at the same time would afford the means of adverting to the high and low water marks as a secondary and additional reference.

3. The rise of the Thames from the sill of the lock at Brentford to the River Kennet near Reading, and from thence to the sill of the lock of the Kennet and Avon Canal at Newbury. This would connect the Kennet and Avon, the Wilts and Berks, the Thames and Severn, and the Somerset Coal Canals.


Scale of Feet
0
100
1
2 Birmingham
3 Coventry
Oxford
5 Gd. Junction
6 Gd. Trunk
7 D. of B-waters
8 Worcester
9 Stratford
10 Dudley
11 Stourbridge
12 Warw. & Birm
13 War. & Napton
14 Gd. Union
15 Union
16 Leicester
17 Loughboro
Summit at W.hampton & Smethwick 0.0.
Birm. 19.10 ¾
Fazeley Junction 100. 3. ¾
Digbeth & Warw. Canal 136. 7.
Summit at Ansley 133. 0
Sum. at Claydon 94. 7. ¾
Daventry 80. 10. ¾
Branch
Tring 2nd Sum. 93. 10. ¾
1st Sum. 113. 10. ¾
Marsworth 138. 10. ¾
Stanley Moss Lock 10. 2 ½
Branch
Froghall 50 7. ½
Sum. at Etruria 61 9 ¾
Birm. Oak
Tardebig
Kingswood
Sum. Gosty Green 0. 0.
Selly Oak 19. 10. ¾
Black Delph 110. 0.
Knowle 94. 7
Digbeth 136.
Hatton
Summit 59. 9. ¾
Ashby de la Zouch 58. 9
Cloud Hill
Swarkeston



Section of the Line of Canal from the River Mersey at Runcorn Gap to the River Thames at Brentford, with the Branch to Paddington Wharfs, 1816.

Engraved for Dr. Thomson's Annals - [illegible]

4. The rise of the Severn from Stourport to Shrewsbury, to connect the Ellesmere, the Montgomeryshire, and the Chester Canals.

5. The difference of level between the Lancaster, and the Leeds and Liverpool canals, where they intersect each other, which seems to be about two miles north of West Houghton.

6. The fall of the River Aire from Leeds to the sea.

7. The fall of the Thames from Brentford to the sea.

8. The fall of the Severn from the sill of the canal lock at Worcester, to the Avon at Tewkesbury, thence to Gloucester, and from Gloucester to the sea.

9. The fall of the Avon from the Kennet and Avon Canal lock at Bath to Bristol Bridge, and thence to the Severn.

10. The fall of the River Nen where it joins a branch of the Grand Junction Canal at Northampton, to Peterborough, and from thence to the sea.

11. If in the reply to the 6th, 7th, 8th, 9th, and 10th inquiries, a reference were made to *fixed points*, contiguous to those estimated to be the high and low water marks, it would afford as great an approximation to accuracy as circumstances will admit; and probably the barometrical admeasurement, conducted with all the proper precautions, may be the most easily adopted in most of these cases.

12. Mr. Rennie, in a plan surveyed under his direction by Mr. Crossley in 1791, states the lockage of the Rochdale Canal to be on the east side 275 feet, and on the west 438½ feet; and Mr. Sutcliffe, in his Treatise on Canals, says, rise on the east side 358 feet, fall on the west 521 feet. Does this difference arise from the lockage of the canal having been increased, to avoid a tunnel or deep cutting at the summit?

I am, Sir, yours very respectfully,
SAMUEL GALTON.

Reference to the Chart of a Synoptic View of the Levels of 18 *Canals, in Reference to the Summit of the Birmingham Canal: to which is added the Authority of each Survey.*

	RISE.		FALL.	
	Feet	Inches	Feet	Inches
I. The Staffordshire and Worcestershire Canal commences in the River Severn at Stourport, unites with the Stourbridge Canal at Stourton, with the Birmingham Canal at Autherley, and falls into the Grand Trunk Canal at Heywood, Staffordshire.				
Rise from Stourport to Autherley	294	8		
Fall from thence to Heywood			100	6
Total........	294	8	100	6
(From Cary's Canal Plans, assisted by a statement from the Company's clerk.)				

	Rise.		Fall.	
	Feet	Inches	Feet	Inches
II. The Birmingham Canal commences in the summit of the Staffordshire and Worcestershire Canal, at Autherley, connects with the Wyrley and Essington Canal, near Wolverhampton, with the Dudley Canal at Tipton Green, the Worcester Canal at Birmingham, the Coventry Canal at Fazeley, and terminates in the detached part of the Coventry Canal at Whittington Brook.				
Rise from Autherley to Wolverhampton	132	0½		
Fall to Fazeley and Whittington Brook........			264	10½
Total........	132	0½	264	10½
The Digbeth Branch joins the Warwick Canal at Digbeth, and falls			36	3¼
(From a survey by T. Bagot, surveyor, Birmingham, 1814.)				
III. The Coventry Canal commences in the Birmingham Canal at Fazeley, joins the Oxford Canal at Longford, and proceeds on a level from thence to Coventry.				
Rise from Fazeley to Longford	96	1½		
The detached part proceeds from Whittington Brook to the Grand Trunk Canal at Fradley Heath, and is level.				
(From a survey by the Birmingham Canal Company's clerk at Fazeley.)				
IV. The Oxford Canal commences in the Coventry Canal at Longford, joins the Warwick and Napton Canal at Napton, the Grand Junction Canal near Braunston, proceeds from thence to Claydon, and falls into the River Isis at Oxford.				
Rise from Longford to Claydon	74	1¼		
Fall to the Isis			195	3½
Total........	74	1¼	195	3½
(From the clerk to the Company.)				
V. The Grand Junction Canal departs from the Oxford Canal near Braunston, proceeds to Braunston, and unites with the Grand Union Canal near Long Buckby, from thence it passes on to Wolverton, to Tring, and falls into the Thames at Brentford.				
Rise from the Oxford Canal to Braunston	36	0		
Fall to Wolverton			137	6
Rise to Tring................................	157	6		
Fall to the Thames at Brentford			395	0
Total........	193	6	532	6
Daventry Branch rises	54	0		
Northampton ditto to the River Nen falls......			118	0

	RISE.		FALL.	
	Feet	Inches	Feet	Inches
Buckingham Branch rises	17	0		
Aylesbury Branch falls			96	0
Wendover Branch from the Tring summit, level.				
Paddington Branch, level.				
(From the Company's clerk, per Netlam and Francis Giles, surveyors.)				
VI. The Grand Trunk Canal commences in the River Trent at Shardlow, joins the Derby Canal near Swarkstone, the detached part of the Coventry Canal at Fradley Heath, the Staffordshire and Worcestershire Canal at Heywood, then proceeds to Etruria, and terminates in the Duke of Bridgewater's Canal at Preston Brook.				
Rise from Shardlow to Etruria	316	3		
Fall to Preston Brook			326	2
Total	316	3	326	2
The Uttoxeter Branch rises, to Stanley Moss	75	0		
And falls to Uttoxeter			192	10
Total	75	0	192	10
(Plan belonging to the Grand Trunk Canal Company.)				
VII. The Duke of Bridgewater's Canal commences in the Rochdale Canal at Manchester, joins the Grand Trunk Canal at Preston Brook, and falls into the River Mersey at Runcorn.				
Fall from Preston Brook to Runcorn			84	0
Branch to Leigh, level.				
(From Cary's Canal Plans.)				
VIII. The Worcester Canal departs from the Birmingham Canal at Birmingham, connects with the Dudley Canal Branch at Selly Oak, the Stratford Canal at Kingsnorton, and falls into the River Severn at Diglis, near Worcester.				
Fall			428	0
(From the Canal Company's clerk.)				
IX. The Stratford Canal commences in the Worcester Canal at Kingsnorton, joins the Warwick Canal by a short level branch from Kingswood, and falls into the Avon at Stratford-upon-Avon.				
Fall from Kingsnorton to the bed of the Avon			338	8½
(From the clerk to the Company.)				
X. The Dudley Canal leaves the Birmingham Canal at Tipton Green, and falls into the Stourbridge Canal at Black Delph.				

	RISE.		FALL.	
	Feet	Inches	Feet	Inches
Fall from Tipton Green to Black Delph			116	0
A branch to the Worcester Canal at Selly Oak, level.				
(From Cary's Canal Plans.)				
XI. The Stourbridge Canal leaves the Dudley Canal at Black Delph, and falls into the Staffordshire and Worcestershire Canal at Stourton.				
Fall from Black Delph to Stourton			182	2½
(From the clerk to the Dudley Canal Company.)				
XII. Warwick and Birmingham Canal commences in the Digbeth Branch of the Birmingham Canal at Digbeth, is joined by a short cut from the Stratford Canal at Kingswood, by the Warwick and Napton Canal near Warwick, and terminates at Warwick.				
Rise from Digbeth to the summit..............	42	0		
Fall to Warwick			188	0
Total........	42	0	188	0
(From the clerk to the Company.)				
XIII. The Warwick and Napton Canal branches from the Warwick and Birmingham Canal near Warwick, and proceeds to the Oxford Canal at Napton on the Hill.				
Fall to Leamington			14	0
Rise from thence to Napton	146	8¼		
Total........	146	8¼	14	0
(Compared with the route by Coventry.)				
XIV. The Grand Union Canal commences in the Braunston summit of the Grand Junction Canal, and terminates in the Union Canal at Foxton.				
Rise from Braunston to the summit	56	3		
Fall to Foxton			75	0
Total........	56	3	75	0
(From N. and F. Giles, surveyors.)				
XV. The Union Canal connects with the Grand Union Canal at Foxton, and falls into the Leicester navigation at Westbridge, Leicester.				
Fall from Foxton to Leicester			160	0
(From B. Bevan, engineer.)				
XVI. The Leicester Navigation proceeds from Leicester to Thringston Bridge, joining in its course the Melton Mowbray Navigation, and the Loughborough Navigation at Loughborough.				

	RISE.		FALL.	
	Feet	Inches	Feet	Inches
Fall from Leicester to Loughborough..........			50	0
Rise (query if not by a rail road) to Thringston Bridge....................................	185	0		
Total........	185	0	50	0
(From a plan by C. Staveley, 1790. The rise of the railway from a survey in 1785, belonging to the Birmingham Canal Company.)				
XVII. The Loughborough Navigation leaves the Leicester Navigation at Loughborough, and falls into the River Trent at Sawley.				
Fall			41	0
XVIII. The Ashby-de-la-Zouch Canal departs from the Coventry Canal near Griff, proceeds to Ashby-de-la-Zouch, and terminates at Ticknall, Derbyshire.				
Rise to Ashby-de-la-Zouch....................	140	0		
Fall to Ticknall			84	0
Total........	140	0	84	0
(From a plan by Whitworth and Smith, 1792.)				

REFERENCE TO PLATE LXIV.

The figures in the perpendicular lines show the number of feet above Runcorn.

From Fradley Heath to Whittlington Brook is a detached part of the Coventry Canal.

Thence to Fazeley is a part of the Birmingham Canal.

	Length.		Rise.		Fall.		Number of Locks.
	M.	F.	Ft.	In.	Ft.	In.	
From Runcorn, to							
Preston Brook, per Duke of Bridgewater's Canal	6	—	84	—			10
Fradley Heath, per Grand Trunk....	66	7	326	2	200	1	58
Fazeley, per Coventry and Birmingham........................	10	4					
Hawkesbury Lock, per Coventry....	21	1	96	1½			13
Braunston Junction, per Oxford	33	7	18	10¼			3
Bull's Bridge, per Grand Junction .... 87 2 Brentford.................. 5 7	93	1	193	6	532	6	101
Total......	231	4	718	7¾	732	7	185

From Bull's Bridge to Paddington Wharf, 13½ miles, and level.

ARTICLE II.

Results of a Meteorological Register kept at the Observatory of the Naval Academy, Gosport, in 1816.
Lat. 50° 47′ 58″ *N.* *Long.* 1° 6′ 4″ *W.* *In Time,* 4′ 24·3″. By William Burney, Esq.

1816.	BAROMETER.				THERMOMETER.							WINDS.					WEATHER.					
Months.	Max.	Min.	Medium.	Greatest variation in 24 hours.	Max.	Min.	Medium.	Greatest variation in 24 hours.	Medium at 8 A.M.	Medium at 2 P.M.	Medium at 8 P.M.	N. to E.	E. to S.	S. to W.	W. to N.	Total number of days.	A clear sky.	Fine, cloudy, &c.	Rainy.	Total number of days.	Evaporation in inches, &c.	Rain in inches, &c.
	Inches	Inches	Inches	In.								days	days	days	days		days	days	days			
January...	30·40	28·99	29·639	0·54	51°	22°	34·84°	17°	—	—	—	5	8	10	8	31	5	14	12	31	0·58	2·18
February..	30·37	28·82	29·842	0·53	54	08	32·07	21	—	—	—	5	3	11	10	29	6	16	7	29	0·77	2·21
March	30·28	29·00	29·742	0·63	55	27	37·95	20	—	—	—	4	10	9	8	31	5	15	11	31	1·39	2·49
April	30·08	29·20	29·690	0·42	72	28	45·22	26	45·73°	54·53°	46·10°	7	11	3	9	30	6	16	8	30	1·42	1·56
May	30·16	29·21	29·814	0·39	74	30	51·63	24	52·48	61·32	50·80	9	4	7	11	31	5	14	12	31	1·85	1·91
June	30·14	29·41	29·922	0·41	78	38	57·50	26	58·00	67·73	55·90	4	4	7	15	30	4	17	9	30	3·27	1·26
July	29·96	29·38	29·656	0·31	76	46	59·60	22	58·93	68·48	56·71	3	5	10	13	31	3	9	19	31	4·58	5·13
August....	30·35	29·20	29·999	0·80	74	47	57·50	23	57·64	69·26	56·71	7	2	10	12	31	4	17	10	31	3·73	3·29
September.	30·40	29·51	30·041	0·49	75	36	54·60	25	55·30	65·13	55·20	4	8	6	12	30	4	18	8	30	2·42	1·68
October ..	30·33	29·25	29·930	0·38	69	36	53·50	20	51·84	60·39	51·96	3	10	8	10	31	3	16	12	31	1·38	3·38
November .	30·75	28·97	29·885	0·73	58	28	42·65	18	39·63	47·76	41·57	8	3	7	12	30	4	16	10	30	0·72	2·70
December..	30·76	28·98	29·880	0·86	53	22	41·17	19	38·19	45·19	41·68	5	3	11	12	31	4	12	15	31	0·47	4·76
	30·76	28·82	29·837	0·86	78	08	47·35	26	50·86	59·97	50·74	64	71	99	132	366	53	180	133	366	22·58	32·55

ANNUAL RESULTS.

Barometer.

	Inches.	*Wind.*
Highest observation, Nov. 30	30·76	N. N. E.
Lowest ditto, Feb. 7	28·82	N. E.
Greatest variation in 24 hours, Dec. 19	0·86	
Annual mean barometrical pressure	29·837	

Thermometer.

		Wind.
Highest observation, June 25	78°	W. by N.
Lowest ditto, Feb. 9	8	N. E.
Greatest variation in 24 hours, June 12	26	
Annual mean temperature	47·35	

Winds.

	Days.
N. to E.	64
E. to S.	71
S. to W.	99
W. to N.	132
	366

Weather.

	Days.
A clear sky	53
Fine, cloudy, foggy, &c.	180
Rain, hail, snow, &c.	133
	366

Evaporation.

	Inches.
Greatest quantity, in July	4·58
Smallest ditto, in December	0·47
Total quantity for the year	22·58

Rain.

	Inches.
Greatest quantity, in July	5·13
Smallest ditto, in June	1·26
Total quantity for the year	32·55

Explanation of the Table.

The barometer is hung in the Observatory, about 30 feet above the level of the sea: and the thermometer, on Six's construction, is placed in a northern aspect, out of the sun's rays, 12 feet above the garden ground. The pluviameter stands clear of all obstructions on the top of the Observatory, which is about 22 feet above the garden ground. The chasm in January, February, and March, of the mean of the thermometer at eight, two, and eight o'clock in the day, is owing to the observations not having been taken regularly during that period: but this does not affect the annual mean temperature. For brevity's sake, the four cardinal-points only are put down in the table to show the direction of the prevailing winds; and the number of days which the winds have blown from each quarter in each month are selected with tolerable accuracy from our monthly journals. The 53 days denominated a clear sky, are those in which the sun has shone forth in all his splendour, without any apparent cloud; the 180 fine, cloudy, &c. are those in which different modifications of cloud have presented themselves to the observer, so as frequently to intercept the rays of the sun: and the 133 rainy days are those in which rain has fallen, and that more than the $\frac{1}{100}$th part of an inch in depth in the space of 24 hours: it should, however, be remarked, that many of this number have turned out fair and cloudy days.

Article III.

On the Chemical Compounds of Azote and Oxygen: and on Ammonia. By John Dalton.

(Read before the Literary and Philosophical Society of Manchester, Oct. 18, and Dec. 27, 1816.)

Azote and oxygen are two of the most important chemical elements with which we are acquainted. In their most simple state they are exhibited to us in the form of elastic fluids, and possess various highly interesting properties, which may be seen in the elementary books of chemistry, and which it is not my present business to enumerate. These two elastic fluids may be mixed together in any proportion, and soon become mutually diffused through each other without manifesting any mark of chemical union, except the simple circumstance of uniform diffusion. Mixed in the proportion of 100 measures of azote to 26½ of oxygen, they constitute the principal part of that great and voluminous mass of elastic fluids—the earth's atmosphere. Lavoisier first demonstrated the constituents of the atmosphere about 30 years ago. He does not seem to have had a clear idea of the kind of union between the two elements; for he uses the terms *combination* and *mixture* indifferently when speaking of the constitution of the atmosphere: thus, he mentions "the mutual adhesion of the two constituent parts of the atmosphere;" and "there still remains a portion of respirable air united to the azote which the mercury cannot separate," &c. (Elements of Chemistry, English translation, fourth edit. p. 86.) This language plainly indicates chemical union. In another place he says, "the azotic gas may be procured from atmospheric air by absorbing the oxygen gas which is *mixed* with it, by means of a solution of sulphuret of potash," &c. (P. 266.) In the table of binary combinations of azote with simple substances

(p. 264) no mention is made of atmospheric air being one of those combinations. These last observations plainly suggest the idea of simple mixture.

Soon after Lavoisier's work the popular Elements of Chemistry by Chaptal were published. This author has a section "on the mixture of nitrogen and oxygen gas, or of atmospheric air;" and he seems every where to consider it as a *mixture*, in the common sense of the word.

In 1800, Mr. (now Sir Humphry) Davy published his valuable researches relating to nitrous oxide, &c. in which the several compounds of azote and oxygen were ably investigated, the results of which we shall have to mention in the sequel. Mr. Davy was inclined to consider atmospheric air as "the least intimate of the combinations of oxygen and nitrogen," and observes, "that the oxygen and nitrogen of the atmosphere exist in chemical union appears almost demonstrable from the following evidences:—

"1. The equable diffusion of oxygen and nitrogen through every part of the atmosphere; which can hardly be supposed to depend on any other cause than an affinity between these principles.

"2. The difference between the specific gravity of atmospheric air and a mixture of 27 parts oxygen and 73 nitrogen, as found by calculation; a difference apparently owing to expansion in consequence of combination.

"3. The conversion of nitrous oxide into nitrous acid, and a gas analogous to common air by ignition.

"4. The solubility of atmospheric air undecompounded in water."

I may observe here that the last three evidences, though plausible at the time, have since been shown to be without solidity.

In 1802 my essays on the constitution of mixed gases were published, containing an hypothesis to explain the uniform diffusion of gases by mechanical means; on this principle the atmosphere was of course considered as a mixture, and not a combination of its elements.

Soon after this, Berthollet, in his researches into the laws of chemical affinity, announced a new explanation of the phenomena of mixed gases. According to this eminent chemist, there are two species of affinity; the one strong, the other weak: the strong affinity makes bodies combine; the weak one only serves to diffuse them through each other without producing condensation of volume; its effects may be called *solution* or *dissolution.* Of this kind, he conceives, is the mutual action of gases that do not combine, and that it operates just the same upon gases inclined to combination or not; thus a mixture of carbonic acid and hydrogen is subject to this weak or slight affinity just as much as one of oxygen and hydrogen. Something similar to this is maintained by Mr. Murray in his Elements of Chemistry (1806), and by Dr. Thomson in the third edition of his Chemistry (1807). Mr. Gough wrote two essays in the Manchester Memoirs (vol. i. second series, 1805), and some essays in Nicholson's Journal (vol. viii. ix. x. 1804–5), all of which were intended to support the opinion of atmospheric air being a chemical compound. The last mentioned author does not avail himself of the two affinities, the strong and the weak, in order to explain the phenomena of mixed gases.

I have animadverted upon the opinions of some of these authors in the above volumes of the Journal, and more particularly in the first part of my chemistry (1808), in which I have materially modified the mechanical hypothesis of mixed gases first published in 1802. Since 1808 there has not, to my knowledge, been much written on the subject of mixed gases.

Sir Humphry Davy's Elements of Chemical Philosophy appeared in 1812; and it might have been presumed, from the great progress made in chemistry within the last few years, that some addition would have been made to our knowledge of the constitution of the atmosphere since his Researches published in 1800; but I find no allusion to his before-mentioned notion of the air being a chemical compound of azote and oxygen, or to the evidences of it. At p. 231, it is observed, that "if four parts of azote be mixed with about one part of pure oxygen gas, they constitute a mixture resembling exactly atmospheric air;" and as no mention is made of atmospheric air, when treating of the compounds of azote and oxygen, it seems to be tacitly implied that this author no longer considers atmospheric air as a chemical compound.

Dr. Henry introduces the subject of atmospheric air into his Chemistry as follows: "The air of our atmosphere, it appears, from the facts stated in the preceding section, is a mixture, or possibly a combination, of two different gases, viz. oxygen gas and azotic gas."

Lastly, Gay-Lussac has recently written an essay on the compounds of azote and oxygen, amongst which he has not mentioned atmospheric air.

These observations seem to show that the opinions of philosophers are far from being uniform in regard to the nature of mixed gases in general, and of the atmosphere in particular. Indeed, it is difficult to ascertain what is the most prevailing opinion. In 1807 Dr. Thomson made the following observation: "Mr. Dalton considers air as merely a mechanical mixture of the two gases of which it is composed. But all other chemists consider it as a chemical compound." (Chemistry, iv. 68.) Whereas a writer in the *Annals of Philosophy* (1815) observes, that "chemists do not appear to have considered atmospheric air in the light of a compound formed upon chemical principles, or at least little stress has been laid on this circumstance."

Whatever may be the ultimate opinion of chemists respecting the infinite variety of mixtures of azote and oxygen, it is clear, I think, that they never can be classed as compounds of azote and oxygen, along with nitrous oxide, nitrous gas, &c. which possess such peculiarly distinguishing features of chemical compounds. I shall, therefore, dismiss the subject of atmospherical air, and proceed to the acknowledged compounds of azote and oxygen.

It may be proper to state here that the object I have in view is an inquiry into the proportions in which the two elements, azote and oxygen, are found united in the different compounds, rather than to exhibit the physical properties of those compounds, most of which are copiously detailed in elementary books, and to which I have little new to add.

Four compounds of azote and oxygen have been long known and recognized by chemists, viz. *nitrous oxide, nitrous gas, nitrous acid*, and *nitric acid:* to these I attempted, in 1808, to add another, which I denominated *oxynitric acid.* Since that time additional labour has been bestowed on these compounds, by Gay-Lussac, published in the second volume of the Mem. d'Arcueil, 1809; by Davy, published in 1812; again by Gay-Lussac in the Ann. de Chim. 1816; and my own further experience since the publication of the second part of my Chemistry in 1810, which has not yet been published. On these I shall now remark according to the order of time.

In the year 1808 Gay-Lussac suggested a new idea on the combination of gaseous bodies, viz. that one measure or volume of one gas combines with one of another, or with two, three, &c. or some small whole number; an idea evidently agreeing with the atomic system in regard to a body's combining with multiples of another, but having no connexion with it in regard to the relation of the original two volumes. This idea, it was expected, would derive support from the multifarious gaseous compounds of azote and oxygen. Those of nitrous gas and oxygen were, at first view, somewhat averse to the new theory; for it had been proved, in the opinion of some, that oxygen combines with nitrous gas in the proportion of one volume of the former to 1·3 nearly of the latter as a minimum, and to 3·6 volumes as a maximum, and further that there was no very definite or marked intermediate point which could be held as striking or peculiar; in short, that one volume of oxygen might be combined with any intermediate proportion we pleased of nitrous gas between 1·3 and 3·6; but that there were limits not to be exceeded by any known means. A very timely discovery of a new eudiometer by Gay-Lussac completely removed these difficulties, and most admirably supported his hypothesis. By means of it he demonstrated that one measure of oxygen always combines with two of nitrous gas, and in no case with less, when the oxygen is in excess, and forms *nitric acid*; and combines with three measures of nitrous gas, and in no ease with more, when this last is in excess, and forms *nitrous acid.* These conclusions, if true, would have annulled the multiplied labours of all his predecessors in this department of science; but the experiments on which they are founded have not succeeded with any one else, and have recently been entirely abandoned by their author, as will be seen in the sequel.

Mr. Davy, in his Researches in 1800, published the result of an interesting experiment on the combination of oxygen and nitrous gas, the former being in excess, in a receiver previously exhausted. He found that one measure of oxygen united in these circumstances to nearly 2·2 of nitrous gas. In 1810 I published the results of eight similar experiments, made by varying the ratios of oxygen and nitrous gas so as to have each of them more or less in excess. By this method I found that the union of oxygen and nitrous gas was subject to great vicissitudes as well as in eudiometric tubes. One measure of oxygen united with 1·44 nitrous gas as a minimum, and with 2·29 as a maximum, in these eight experiments.

Sir Humphry Davy in 1812 gives an account of repeated experiments on the combinations of oxygen and nitrous gas in exhausted vessels, though the particulars are not detailed; from which he infers that the acid obtained from mixtures of nitrous and oxygen gas over water is never saturated with oxygen, and that the true nitric acid consists of one measure oxygen and 1$\frac{1}{3}$ nitrous gas (that is, 1·5 oxygen and two nitrous); he further infers that one volume oxygen and two nitrous gas constitute 1$\frac{1}{2}$ volume of *nitrous acid gas.* He allows that one measure oxygen unites in certain circumstances with from two to three measures of nitrous gas; but has given no name to this last compound. The limits of combination he considers as one oxygen to 1$\frac{1}{3}$ nitrous as a minimum, and to three nitrous as a maximum. This, as may be seen, agrees nearly with my previous determination; but differs materially from Gay-Lussac's as far as regards the minimum of nitrous gas. One of the above observations of Davy appears to me important, and, as far as I know, original; namely, that nitric acid is constituted of oxygen and nitrous gas, the latter being a minimum according to the methods at present known of uniting them. This notion, I apprehend, is correct; and it has since, as will be seen, been adopted and confirmed by Gay-Lussac. Davy suggests, with probability, that nitric acid cannot be exhibited in the form of a permanent elastic fluid, but always requires some base, as water, an alkali, &c. as necessary both for its formation and preservation. In this respect it seems to me analogous to the sulphuric and muriatic acids. Though differing from Gay-Lussac in regard to some of the compounds of azote and oxygen, he agrees with him in others, and seems in general to adopt the notion of gases uniting in volumes of simple ratios.

In regard to nitrous oxide, Davy agrees with Gay-Lussac that one volume of it contains one of azote and half a volume of oxygen; yet he states the specific gravity of nitrous oxide such that 100 cubic inches weigh 48 or 49 grains, whereas they ought to weigh 46$\frac{1}{2}$ grains from his own data of the composition of the gas. There must, therefore, be an error somewhere in this statement.

They both conclude that nitrous gas is composed of equal volumes of azote and oxygen; this agrees with the modern specific gravity of nitrous gas determined since the theory of volumes was announced, but not with the previous one as found by the experiments of Kirwan and Davy: it certainly would be desirable to have the specific gravity of nitrous gas ascertained without any view to theory.

In the present year (1816) Gay-Lussac has published in the Ann. de Chim. another essay on the compounds of azote and oxygen. In this new essay he has abandoned both his former maximum and minimum of nitrous gas uniting to oxygen, namely, two measures and three measures, as well as his eudiometer; and it would have been satisfactory if he had given some reasons for his being so far misled in regard to phenomena so generally known and received. He now admits the minimum of nitrous gas to be $1\frac{1}{3}$ measure for one of oxygen, the same as Davy acknowledges, and nearly the same as I announced in 1810; but the maximum, he contends, instead of being three measures to one, as he formerly held, or 3·6 as I maintained, is precisely four measures. He follows Davy in adopting the compound of one oxygen and $1\frac{1}{3}$ nitrous gas as constituting nitric acid; one oxygen and two nitrous as nitrous acid gas: but he adds that one oxygen and four nitrous gas form a compound which he denominates *pernitrous acid*, and supposes it has never been noticed by any author before; though I had given a figure of it six years ago, under the denomination of nitrous acid (Chemistry, Plate V. No. 45); and had pointed out distinctly the method of obtaining it in a state of purity (p. 366). In acknowledging his own error in regard to the proportions of nitric acid, he properly adverts to a similar one of mine: in fact, we erred nearly in the same degree in regard to the proportions for nitric acid, having both of us mistaken nitrous for nitric acid; but he is incorrect in the observation that my *oxynitric acid* is in reality the common nitric acid. The oxynitric acid, the existence of which I inferred, consisted of one atom azote and three oxygen; or it consisted of 80 per cent. oxygen by weight: whereas nitric acid, according to Gay-Lussac, contains only 74 per cent. of oxygen, and consists of two atoms or measures of azote and five oxygen. My oxynitric acid is quite different, therefore, from nitric acid, and the necessity of supposing such a compound as oxynitric acid is now superseded by the new view of the constitution of nitric acid.

Since my last publication I have frequently recurred to the subject of nitric acid, as well as to the other compounds of azote and oxygen; and about two years ago I became convinced that what I had called and figured as *nitric acid* was in reality *nitrous acid gas*, and that nitric acid is constituted of two atoms of this last connected by one of oxygen, and is formed by uniting one measure of oxygen to 1·2 of nitrous gas. The weight of an atom of nitric acid is, therefore, I apprehend, 45, and not 38 (the double of 19), as accounted in my Chemistry. Conformably with this idea, I furnished Dr. Henry with a table of nitric acid, which was printed in the seventh edition of his Chemistry last year (1815).

Having fallen into an error, along with my cotemporaries, in regard to the constitution of nitric acid, it is proper to state what I have to allege by way of exculpation, especially as I profess to publish as little as possible but what I can support on my own experience.

It appeared to be well established by the concurring experiments of Kirwan, Richter, and Davy, that the nitrate of potash is constituted of 47 or 48 acid and 52 or 53 potash. My own former experiments did not oppose this conclusion; though it now appears that numbers the reverse of the above are more approximate to the truth. I had previously ascertained the relative weight of the atom of potash to be 42, from a comparison of several salts; and hence that of nitric acid was deduced to be 38. Now one atom of azote and two of oxygen making just 19, I concluded that two such compounds must form the atom of nitric acid united to one of potash, as there did not appear any other way of forming a compound of 38 out of the elements of azote and oxygen. What contributed materially to confirm this conclusion was the near coincidence of the proportion of azote and oxygen with that determined by the celebrated experiments of Mr. Cavendish, as well as its agreement with the results obtained by the electrification of nitrous gas.

Knowing that nitric acid, if so constituted, must be formed of one measure oxygen and 1·8 nitrous gas, I was well aware from experiment that an acid with a greater proportion of oxygen was attainable, and hence inferred the existence of *oxynitric acid*, as well as nitrous acid, which I formed by combining one measure oxygen with 3·6 nitrous gas, or twice the volume necessary for nitric acid. The ideas of these compounds are rendered easily intelligible by the symbols invented for the purpose, and delineated in the fifth plate of my Chemistry.

Subsequent experiments having disproved the accuracy of the fundamental fact on which this theory of nitric acid was formed, it became necessary to modify it accordingly. It now appears, from the best experiments we have, that nitre is formed of 52 parts acid and 48 potash nearly, the ratio of which is that of 45 to 42 nearly. Hence the atom of nitric acid contains two atoms of azote and five of oxygen, or two of nitrous gas and three of oxygen; and it may be formed by uniting one measure of oxygen with 1·2 of nitrous gas, which appears to be the minimum. Or 100 parts of azote by weight take 350 of oxygen to form nitric acid. The compound which I formerly denominated nitric acid, if it exist, which seems probable, may be denominated *nitrous acid gas*, and what I called nitrous acid is the *pernitrous acid* of Gay-Lussac, or, as I should rather call it, *subnitrous acid*, admitting the existence of the other compound.

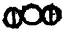
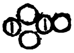
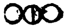
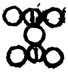
If this explanation be admitted, it is evident we must henceforward consider the compound formed by electrifying a mixture of azote and oxygen, after the manner of Cavendish, as *nitrous acid gas*, and not nitric acid, as it has generally been conceived hitherto; and it must be regarded the same compound as that formed by electrifying nitrous gas.

It may be proper now to state the points of agreement and disagreement in the present views of chemists in regard to the compounds of azote and oxygen. The notion that nitrous gas contains just twice the quantity of oxygen that nitrous oxide does, united to a given quantity of azote, seems universally admitted. Another compound, containing twice as much oxygen as nitrous gas, is also admitted by some, namely, nitrous acid gas. And nitric acid is allowed to contain five times the oxygen that nitrous oxide does. In addition to these four compounds, Gay-Lussac, Berzelius, Thomson, and I, admit a fifth, pernitrous or subnitrous acid, which contains three times the oxygen that nitrous oxide does. This is the compound I formerly called *nitrous acid*, and what is so called by Berzelius and Thomson. It will still be the proper name, if the existence of an intermediate compound between this and nitric acid cannot be established. Of this more in the sequel. Berzelius has some peculiar notions in regard to azote. He conceives that azote is formed of oxygen and a substance he calls *nitricum*; and that azote + a given quantity of oxygen forms nitrous oxide, and azote − the same quantity of oxygen leaves nitricum.

The subject of greatest difference amongst us is in regard to the absolute weights of the elements azote and oxygen which combine to form the several compounds. Gay-Lussac, and most of the other chemists I have mentioned who follow him as volumists, contend that the proportions are as under, viz.:—

Measures.		Measures.		Measures.	
100 azote	+	50 oxygen	=	100	nitrous oxide
100	+	100	=	200	nitrous gas
100	+	150	=		subnitrous acid
100	+	200	=	100	nitrous acid gas (150, Davy)
100	+	250	=		nitric acid.

But from the views I entertain on the subject as derived from experiments, the true proportions of the compounds would be more nearly stated as under:—

Measures.		Measures.					
100 azote	+	62 oxygen	=	100	±	nitrous oxide	
100	+	124	=	200	+	nitrous gas	
100	+	186	=			subnitrous acid	
100	+	248	=	100	±	nitrous acid gas	
100	+	310	=			nitric acid	

That is, I find 24 per cent., or nearly ¼ more of oxygen in all the different compounds than the above authors.

A disquisition on the grounds of these differences would lead me too much into detail to be pursued on the present occasion; and, besides, I have a train of experiments in view, suggested by Dr. Henry's paper on the analysis of ammonia, which I have reason to think will tend to clear up some remaining obscurities, both with regard to the last-mentioned article, and to the compounds of azote and oxygen. If these should succeed, they may afford a subject for an appendix to the present essay on a future occasion.

ARTICLE IV.

A further Continuation of the Observations made by burning a highly compressed Mixture of the Gaseous Constituents of Water. In a letter to the Editor by Edward Daniel Clarke, LL.D. Professor of Mineralogy in the University of Cambridge, and Member of the Royal Academy of Sciences at Berlin, &c.

(To Dr. Thomson.)

SIR,

The increasing powers of the gaseous blow-pipe, which I had called Newman's, as being made by that artificer, but which I now find to have been invented by Mr. Brooke,* require some further observations. The improvement, suggested by Professor Cumming, of a pneumatic cylinder for containing *water*, for the passage of the gas, has tended greatly to ensure the safety of the operator; but it has also caused the failure of certain experiments for the reduction of the *earths* to the *metallic* state; as it might have been obvious to every chemist, who is sensible of the effect likely to be produced by humid gases, and by the action of vapour, and of occasional sallies of water, from the jet of the apparatus, when so constructed, upon substances exercising a powerful attraction for *oxygen*. The absolute necessity of removing such an obstacle to success in these experiments, induced me to make trial of *oil*, as a substitute for *water*, in the pneumatic cylinder; and the consequences have surpassed my most sanguine expectations. The ebullition is thereby rendered more tranquil; the heat of the ignited gas sustains no diminution; it is propelled in a more desiccated state, and the safety of the apparatus is greatly augmented. In the experiments with *water* in the cylinder, the occasional detonations, which took place above the fluid A B, sometimes forced the water into the tube *x y*; or, if the diameter of the jet, *m n*, were much increased, forced it into the reservoir; rendering an explosion of the whole apparatus extremely probable.

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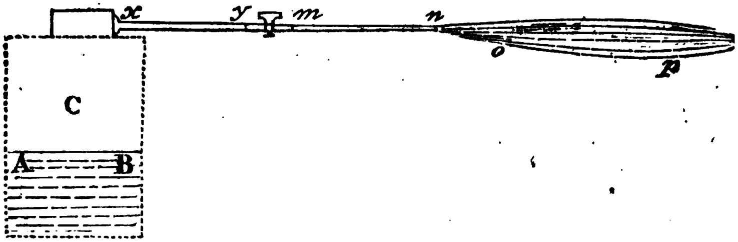
* See the description of it, by Mr. Brooke himself, *Annals*, vii. 367.



But when *oil* is substituted as the fluid represented by A B, accidents of this kind are not likely to happen. I have purposely exploded the gas in the chamber C upwards of 20 times; to prove the instrument. The *oil*, in these trials, was never driven from its place; neither did the combustion of the gas cause it to take fire. Encouraged by the security it offered, I have increased the diameter of the tube *m n*, until it now equals $\frac{1}{25}$ of an inch; and the consequence of this is, that the body of the flame represented by *o p*, is of magnitude sufficient to act upon 100 grains of platinum, which are instantly fused by its intense action; and by dropping minute pieces of the metal into the boiling and burning mass, its bulk may be increased. Having commenced therefore with stating, as a result of the intense heat of the ignited gas, that globules of melted *platinum* were obtained weighing five grains,* you will judge of the increased power of the blow-pipe which has enabled me to obtain masses of the same metal in fusion weighing from 100 to 150 grains, and upwards.† The combustion of *iron*, under the same circumstances, affords so beautiful a phenomenon, that I can recollect no instance of any chemical experiment to be compared with it; and when the two metals, *platinum* and *iron*, are fused together in a *charcoal* crucible, before the ignited gas, their joint combustion affords a pleasing and brilliant fire-work.

But in describing the apparatus as being perfectly secure, there are, of course, certain cautions to be observed. In using tubes with such large diameters as I have now mentioned, partial explosions of the gas within the safety cylinder, C, will more frequently happen, and the detonations will be more powerful; therefore, in these cases, it is always necessary to examine the cylinder for the purpose of seeing that the oil has not been forced into the reservoir; but remains at its proper level, A, B. If this be neglected, the operator will be liable to the consequences of an explosion from the reservoir. An accident of this kind occurred while I was engaged with Professor Cumming in making experiments. There had been four pretty loud detonations, owing to the explosions of the gas in the safety cylinder; until at length the oil at A B was driven out, and the wire-gauze in the cap above C was broken. Presently, as Professor Cumming was turning the stop-cock of the jet, the whole of the apparatus exploded; and the consequences might have been very serious, if we had not been protected by the skreen which I described upon a former occasion. The operator should also constantly bear in mind the indispensable caution of never opening the stop-cock, below the piston, before he has withdrawn the handle of the piston, when he proposes to introduce a fresh supply of gas into the reservoir. Using these precautions, the apparatus is perfectly safe.

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* See Journal of the Royal Institution, iii. 107.

† It was desirable that a careful estimate should be made of the specific gravity of platinum, in this pure state, after fusion, and after having sustained diminution by combustion for some time, in order that all impurities might be driven off. For this purpose we selected a brilliant globule of the metal, weighing 73 grains; and, having previously extended it as much as possible by hammering, that every air bubble might be removed, its specific gravity was ascertained in distilled water at a temperature of 63° of Fahrenheit both by Professor Cumming, and by me, and found equal to 29·857.

Having thus briefly described the means by which I have been enabled to increase the powers of this blow-pipe, and to add to its security, I shall subjoin a statement of the few additional experiments made with it which may be worth your notice. The interest which has been excited by the accounts already published of the results thereby obtained is mainly due to you; but it has given rise to repeated inquiries respecting the person to whom the invention is due, of burning the two gases in a state of condensation from a *common* reservoir. I have already mentioned,[*] that their first application to aid the operations of the blow-pipe was made in 1802, by an American; but, in their first application, the gases were propelled from *different* reservoirs. I believe I may now add, that so long ago as the period above alluded to, you made use of these gases yourself, for the same purpose; condensing them by means of a column of water, and igniting them in a compressed and mixed state, as propelled from a *common* reservoir; and that the frequent explosion of your apparatus put a stop to your experiments. You will correct me if there be any error, therefore, in considering you as the first person who made use of the mixed gases, as I have now used them. The proportion, observed in the mixture, is all that I can lay claim to; which instead of consisting of *hydrogen* in *slight* excess with *oxygen*,[†] or of the gases in equal parts, has always been, by bulk, *two* parts of *hydrogen*, at the least, to *one* of *oxygen*; and, in some instances, I have made the excess greater on the side of the *hydrogen*; using it in the proportion of *three* to *one*. It is to this excess on the side of the *hydrogen*, that I am to attribute the decomposition of the *earths*, and their reduction repeatedly, to the *metallic* state. For the truth of their decomposition, I may appeal to the testimony of the most experienced chemists now resident in this

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[*] Journal of the Royal Institution, No. III. p. 105, note.

[†] The mixture of *hydrogen* in *slight* excess with *oxygen*, was tried by Sir H. Davy (see Journal above cited, p. 127). He considered it as producing a more intense heat from combustion than any other flame he had examined. It is, however, greatly inferior to the heat produced by burning these gases when mixed in the proportion for forming *water*; as the results have proved.

University; but as it has been confirmed by your high authority,* the evidence will be deemed sufficient "by all who are willing to give a peaceful entrance to truth." I have sometimes altered the nature of the gases, and have often tried the effect of varying the proportion between them; but the greatest degree of heat has always been produced, if the gases be perfectly pure, by mixing them exactly in the proportion for forming *water*. A greater excess of *hydrogen*, as it renders the mixture less explosive, always proportionally diminishes the heat. With Professor Cumming, I made trial of the *carburetted hydrogen* gas in a state of mixture with *oxygen*. Its combustion was characterized by a flame of a sapphire-blue colour; but the heat was barely sufficient for the combustion of *iron* wire. *Platinum* was not melted by it. The Professor himself had previously tried a mixture of the *super-carburetted hydrogen*, or *olefiant* gas, with *oxygen*; but found the heat also defective. In the following experiments, the gases were mixed according to the proportions originally suggested by experiments for the formation of *water*, and by the phenomena, attending its decomposition, which I had myself witnessed during a long residence upon Mount Vesuvius.

Continuation of the Experiments.

1. *Muriate of Barytes.*—As this substance has been supposed by Sir H. Davy to be a compound of the *metal* of *barytes* with *chlorine* in the proportion of 0·66 of the *metal*, to 0·34 of *chlorine*, I wished to effect the volatilization of the *chlorine*, for the developement of the *metal*. The experiment succeeded; but it often fails; owing to the volatilization of the minute globules of *metal* in the moment of their revival. The plan I adopted was, to place the *muriate* first upon *charcoal*, and after its ebullition ceases to collect the dry mass, together with some of the *charcoal*, and again expose the whole to the ignited gas in a *charcoal* crucible. In this manner I observed the brilliant globules of the metal dispersed upon the surface of the *charcoal*, as they were exhibited to Mr. Holme and to me in a former experiment with the *nitrate of barytes*; and as he has since described their appearance;† but in this instance they were so exceedingly minute that I could not succeed in my endeavours to place them, as before, in *naftha*.

I succeeded however in obtaining very large globules of the metal of *barytes* in the highest state of metallic lustre of which any metal is capable, and which for a long time were preserved in *naftha*, and exhibited to Professor Cumming, to Mr. Holme, and to other chemists of this University, by a different process. I mixed the two gases in the proportion of three parts of *hydrogen*, by bulk, to one part of *oxygen*. The heat in this case is less intense, but the deoxidating power may perhaps be greater. I exposed some pure *barytes* to the flame of this gaseous mixture supported in forceps made of *slate*. The *metallic* base of the *earth* was revived in such perfection that, after I had placed it in *naftha*, Professor Cumming compared it to *platinum* and to *mercury*. Mr. Holme also saw it often, and recognised the brilliant metallic appearance he had before witnessed, and which he has described in the *Annals*, vol. viii. As this metal retained its lustre unaltered for more than three weeks in *naftha*, it was taken out, and the mass containing it was placed in a covered *platinum* crucible, and left exposed for some days to the action of the air. At the end of this time all metallic lustre had disappeared; but there remained some hard lumps of the earth within the crucible. Distilled water was then added, and the whole, falling into the pulverulent form, was collected upon a filter; proving to be nothing more than pure *barytic* earth.

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* See *Annals*, ix. 7.

† See Mr. Holme's observations upon the reduction of *barytes* to the *metallic* state, *Annals*, viii. 471.

2. *Muriate of Rhodium.*—A small quantity of this *salt*, of a red or rosy colour, had been given to me by the Rev. Archdeacon Wollaston, when Professor of Chemistry in this University; having himself received it from his brother. Its purity, therefore, may be inferred. Being placed in a *charcoal* crucible, it admitted of easy fusion, attended with occasional combustion. The metal was then revived. It appeared at first black, externally, like the *metallic* slag of *barytes*. Upon being exposed again to the ignited gas it began to boil vehemently, and was partly volatilized. There then remained a brilliant globule of metal resembling the purest *platinum*. This metal was malleable; but when much extended by a common hammer upon an anvil, it separated. By further continuance of the heat it was entirely volatilized. I then repeated the experiment; and again obtained the metal in a malleable state; in this state it was sent to Dr. Wollaston; after being hammered.

3. *Brittle Regulus of Rhodium.*—Having received some of this substance from Dr. Wollaston, I expected to render it malleable by the action of the ignited gas; but found it to be impracticable; owing to some impurity with which it was combined, and which no degree of heat would totally expel. It appeared to be dissipated in white fumes; but these were owing to the volatilization of the metal; the residue, after fusion, being always brittle.

I then endeavoured to purify it, by solution in the *nitro-muriatic* acid; using nearly Dr. Wollaston's proportion of two parts of *muriatic* acid to one of *nitric*; and first fusing the *rhodium* with four times its weight of *lead*, by means of a common blow-pipe. The solution was not entirely effected; owing to a deficiency in the proportion between the two acids; but being evaporated to dryness a salt was obtained which, after solution in *alcohol*, yielded a yellow precipitate to pure *ammonia*. This precipitate when fused before the ignited gas became extremely malleable, but it was found to consist of *rhodium* still combined with *lead*. A further continuance of the heat, placing the compound upon *charcoal*, at length volatilized or vitrified the *lead*, and the *rhodium* was obtained in a malleable state. Professor Cumming, who, with Mr. Powell, and Mr. Holme, were present at these experiments, himself beat out the *rhodium*, which had been obtained in form of a globule, into a thin circular lamina of the metal.

4. *Soda-Muriate of Iridium and Osmium.*—I suppose this salt to be a *soda-muriate.* It was found in the chemical laboratory by Professor Cumming, in the form of a black powder resembling that of *plumbago,* and was contained within a small phial which the late Professor Tennant had labelled "*Iridium and Osmium.*" My reason for particularly noticing it is this; that it contains the most infusible body I have ever met with. When exposed to the action of the ignited gas, brilliant metallic globules are dispersed upon the *charcoal* used as a support, and a brownish angular residue is left, unreduced, which sustains no change, nor even alteration of form, or colour, by any further continuance of the heat.

5. *Ore of Iridium and Osmium in Grains.*—Some of these grains being placed in a *charcoal* crucible, were fused with difficulty into one globule; a combustion of the *iridium* taking place the whole time; accompanied by an evident volatilization. The globular residue was afterwards flattened upon an anvil by severe shocks of a hammer. The metal however proved to be so exceedingly hard that it was only partially extended by this pressure. I afterwards endeavoured to file it; but the sharpest *Carron* files would scarcely touch it. Constant friction during 30 minutes was necessary to disclose an even surface of metal. It then exhibited a very high degree of metallic lustre; inferior only to that of *palladium* alloyed with *nickel.*

6. *Alloy of Silicium and Iron.*—This alloy is easily obtained, by placing a bead of pure *silex,* after fusion, into contact with a bead of pure *iron* of equal bulk, in a *charcoal* crucible, and fusing them together. The *iron,* at this exalted temperature, takes all the *oxygen* of the *silex,* and a white metal is developed, consisting almost wholly of *silicium,* combined with a little *iron.* The colour of the alloy resembles precisely that of pure *silicium* obtained by the reduction of the earth.

7. *Reduction of Wood Tin and Barytes.*—I have placed the reduction of these two substances together, in order to exhibit, by an easy analogical process, the decisive evidence thereby afforded of the *metallic* base of *barytes,* and its developement, when the pure *earth* is exposed, *per se,* to the action of the ignited gas. This takes place equally, whether the *slag* of *barytes* be supported by forceps made of slate, porcelain, pipe-clay, or of iron. For the sake of noticing the nature of the deposit made upon polished iron, by the two substances which were the subject of the experiment, iron forceps were employed in the present instance. The forcible analogy of the results cannot fail to strike the attention of every chemist. In both trials the coloured flame seems immediately to precede the revival of the metal.

(A.) *Wood Tin.*—Fusion—deposition of a white oxide on the iron forceps—violet-coloured flame—scintillation, denoting combustion

—escape of white fumes—slag, of a jet-black colour, disclosing metallic lustre to the action of the file.

(B.) *Pure Barytes.*—Fusion—deposition of a white oxide on the iron forceps—chrysolite-coloured flame—scintillation, denoting combustion—escape of white fumes—slag of a jet-black colour, disclosing metallic lustre to the action of the file.

8. *Common Black Oxide of Manganese.*—Some of this substance taken from an iron retort, after having been partially deoxidated in the preparation of *oxygen* gas, was mixed with *oil*, and exposed to the ignited gas in a *charcoal* crucible. It became speedily fused into a dark globule, which, when filed, exhibited a brilliant white metallic lustre, resembling that of the *metal* of *barytes*.

9. *Granular Tin of the Molucca Isles.*—I received this substance from Professor Thunberg, at Upsal, in Sweden. It is in the form of black grains which are octahedrons. Placed on *charcoal*, they were speedily fused, and their fusion was accompanied by a violet-coloured flame, which immediately preceded the revival of the metal, in a malleable state.

10. *Green foliated Oxide of Uranium from Cornwall.*—Upon the first action of the ignited gas the *green* colour disappeared. The *uranium* then became *white*. Fusion ensued; attended with a slight but decisive smell of *sulphur*. The substance then exhibited a vehement ebullition, accompanied by combustion, resembling that of *iron*. The revival of the metal followed, in the form of a dark reddish brown globule; which, when filed, exhibited a metallic lustre not unlike that of *iron*. It was brittle, and seemed to be one of the hardest of metals.

11. *Experiments with Nickel.*—A brittle regulus bearing the name of this metal, had been purchased of Mr. Knight, in Foster-lane. It was exposed to the action of the ignited gas upon a *charcoal* support, and a copious disengagement of *arsenical* fumes immediately ensued; filling all the room with the peculiar odour of *arsenic*. Fusion and combustion followed; and the combustion continued to be exhibited by the melted metal, even after the stop-cock of the jet was closed, and the gas extinguished. The residue was a brittle metallic globule. Various attempts were afterwards made to obtain the metal in a malleable state, but without success; by repeatedly dissolving it in diluted *nitric* acid, and evaporating the solution to dryness; adding the usual process of a subsequent solution and evaporation by means of pure *ammonia*. The smell of *arsenic* during its fusion was always perceptible, and the residue as constantly brittle. This is remarkable; because I obtained the *nickel* in a more malleable state by the fusion of *arsenical nickel*, or *kupfer-nickel*; and the process has been described in the account of my former experiments.* I succeeded however in forming various alloys of this metal, and, among others, the following:—

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* See *Annals*, viii. 362.

12. *Alloy of Palladium and Nickel.*—This beautiful alloy proved to be so far malleable that it admitted of being flattened by a common hammer upon an anvil. When filed and polished, its surface became a perfect mirror; reflecting more light than any metallic substance with which I am acquainted. It would afford a useful and highly ornamental compound in the arts; perhaps surpassing in lustre the most splendid metals known; and might be advantageously employed in making mirrors for telescopes.

13. *Alloy of Nickel with Iron.*—The two metals were fused together in equal parts by bulk. Previously to their union there was a vivid combustion, but it ceased in the instant of their combination. The fusion was afterwards more tranquil, with less of ebullition; the residue being a globule of white and very splendid metal.

Other Alloys obtained by Fusion before the ignited Gas.

14. *Alloy of Palladium and Copper.*—The two metals were fused in equal parts, by *bulk*, and seemed to unite with an avidity as if they exercised a mutual attraction upon each other. After their union, the alloy was remarkably fusible; and its fusion was always attended with a partial combustion of the *palladium.* This alloy is of a pale colour, and easily acted upon by the file; but it is susceptible of a very high polish.

15. *Alloy of Platinum and Copper.*—The metals were here combined in equal parts by *weight.* The alloy is remarkably fusible, and continued in a state of vehement ebullition after the extinction of the gas. It is soft; easily filed; malleable; and of a pale colour resembling that of pure *gold.* Indeed, it seems as if *gold* might be thus imitated, both with regard to weight and to colour.

16. *Alloy of Platinum and Iron in equal Parts by Weight.*—To this alloy I alluded in the beginning of my letter. The combustion of the two metals, when fused together in a *charcoal* crucible, exhibits a very brilliant firework. It is malleable; but so hard that a file will scarcely touch it; after being filed, the surface exhibits a very high degree of lustre.

17. *Alloy of Platinum and Iron in equal Parts by Bulk.*—This alloy is always brittle. In cooling, a cavity is formed, in the centre of the mass, as in the cooling of *bismuth*; and it is studded with a minute but brilliant crystallization.

With these alloys I must for the present conclude my observations. The statement I have already made, in describing them, will show that the valuable work of Lewis,* although generally correct, is inaccurate upon some points. I might extend the detail I have now offered to a much greater length if I were to relate all the experiments which I have made, in forming metallic compounds. Almost every combination mentioned by Lewis, with *platinum*, I have repeated, and many that he has not mentioned,

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* Philosophical Commerce of Arts, Lond, 1763.

The general result of my observations has excited in my mind a hope, that the means I have used will be applied upon a more extended scale to aid the arts and manufactures of this country. By increasing the capacity of the reservoir, and the condensing power of the apparatus, the diameter of the jet may be also enlarged; and the consequence will be, that a power of fusion, the most extraordinary, as a work of art, which the world ever witnessed, may be employed, with the utmost economy both of space and expenditure, and with the most certain safety. As far as *mineral* substances are concerned, the character of *infusibility* is for ever annihilated. Every *mineral* substance, not even excepting *plumbago*, has been fused. There remains, therefore, only one substance, namely, *charcoal*, to maintain this character; and if I have leisure for a subsequent dissertation, I trust I shall be able to show that *charcoal* itself exhibits some characteristics of a fusible body. I must postpone my remarks upon this subject for a season. The duties incumbent upon my public lectures in the University will preclude the possibility of my making any further experiments before the beginning of the next summer; when, if any thing should occur, that may be deemed worthy of insertion in your *Annals*, it shall be duly communicated.

I have the honour to be, &c.

Cambridge, Jan. 23, 1817. EDWARD DANIEL CLARKE.

ARTICLE V.

An Essay on the Oopas, or Poison Tree of Java, addressed to the Honourable Thomas Stamford Raffles, Lieutenant Governor. By Thomas Horsfield, M.D. (Communicated to the Society by the President.)*

I HAVE proposed to myself in the following Essay to offer you a short account of the oopas of Java. I feel some satisfaction in being able, at a time when every subject relating to this island has acquired a degree of interest, to furnish you with a faithful description of the tree, made by myself on the spot where it grows, and to relate its effects on the animal system by experiments personally instituted and superintended; and I flatter myself that the practical information detailed in the following sheets will refute the falsehoods that have been published concerning this subject, at the same time that it will remove the uncertainty in which it has been enveloped.

The literary and scientific world has in few instances been more grossly and impudently imposed upon than by the account of the pohun oopas, published in Holland about the year 1780. The history and origin of this celebrated forgery still remains a mystery. Foersch, who put his name to the publication, certainly was (according to information I have received from creditable persons who have long resided on the island) a surgeon in the Dutch East India Company's service, about the time the account of the oopas appeared.* It would be in some degree interesting to become acquainted with his character. I have been led to suppose that his literary abilities were as mean as his contempt of truth was consummate.

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* From the Batavian Transactions, vol. vii. Published at Batavia in 1814.

Having hastily picked up some vague information concerning the oopas he carried it to Europe, where his notes where arranged, doubtless by a different hand, in such a form, as by their plausibility and appearance of truth, to be generally credited.

It is in no small degree surprising that so palpable a falsehood should have been asserted with so much boldness, and have remained so long without refutation; or that a subject of a nature so curious and so easily investigated, relating to its principal colony, should not have been inquired into and corrected by the naturalists of the mother country.

To a person in any degree acquainted with the geography of the island, with the manners of the princes of Java, and their relation to the Dutch government at that period, or with its internal history during the last 50 years, the first glance at the account of Foersch must have evinced its falsity and misrepresentation. Long after it had been promulgated, and published in the different public journals in most of the languages of Europe, a statement of facts, amounting to a refutation of this account, was published in one of the volumes of the Transactions of the Batavian Society, or in one of its prefatory addresses. But not having the work at hand, I cannot with certainty refer to it, nor shall I enter into a regular examination and refutation of the publication of Foersch, which is too contemptible to merit such attention.

But though the account just mentioned, in so far as relates to the situation of the poison tree, to its effects on the surrounding country, and to the application said to have been made of the oopas on criminals in different parts of the island, as well as the description of the poisonous substance itself, and its mode of collection, has been demonstrated to be an extravagant forgery; the existence of a tree on Java, from whose sap a poison is prepared, equal in fatality, when thrown into the circulation, to the strongest animal poisons hitherto known, is a fact, which it is at present my object to establish and to illustrate.

The tree which produces this poison is called *antshar*, and grows in the eastern extremity of the island. Before I proceed to the description of it, and of the effects produced by this poison, I must premise a few remarks on the history of its more accurate investigation, and on the circumstances which have lately contributed to bring a faithful account of this subject before the public.

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* Foersch was a surgeon of the third class at Samarang in the year 1773. His account of the oopas tree appeared in 1783.

At the time I was prosecuting my inquiries into the botany and natural history of the island on behalf of the Dutch government, Mr. Leschenault de la Tour, a French naturalist, was making a private collection of objects of natural history for the governor of the north-east coast of Java. He shortly preceded me in my visit to the eastern districts of the island, and while I was on my route from Sourabaya in that direction, I received from him a communication containing an account of the poison tree, as he found it in the province of Blambangan. I am induced to make this statement, in order to concede, as far as regards myself, to Mr. Leschenault de la Tour, in the fullest manner, the priority in observing the oopas of Java. I do this to prevent any reflection, in case a claim to the discovery should be made at a future period: but I must be permitted to add, in justice to the series of inquiries which engaged me, and the manner in which they were carried on, that the knowledge of the existence of this tree was by no means uncommon or secret in the district of Blambangan, in the environs of Banyoo-wangee; that the commandant of the place, a man of some curiosity and inquiry, was acquainted with it, and that it could not (in all probability) have escaped the notice of a person, who made the vegetable productions an object of particular inquiry, and noted with minute attention every thing that related to their history and operation.

It is in fact more surprising that a subject of so much notoriety in the district of Blambangan, and of so great celebrity and misrepresentation in every other part of the world, should so long have remained unexplored, than that it should finally have been noticed and described; and since my visit to that province I have more than once remarked the coincidence which led two persons of nations different from each other, and from that which has long been in possession of the island, who commenced their inquiries without any previous communication and with different objects in view, within the period of about six months, to visit and examine the oopas tree of Java.

The work of Rumphius contains a long account of the oopas, under the denomination of arbor toxicaria; the tree does not grow on Amboina, and his description was made from the information he obtained from Macassar.

His figure was drawn from a branch of that which was called the male tree, sent to him from the same place, and established the identity of the poison tree of Macassar and the other eastern islands with the antshar of Java.

The account of this author is too extensive to be abridged in this place, it concentrates all that has till lately been published on this subject: but the relation is mixed with many assertions and remarks of a fabulous nature, and it is highly probable that it was consulted in the fabrication of Foersch's story. It is, however, highly interesting, as it gives an account of the effects of the poisoned darts, formerly employed in the wars of the eastern islands, on the human system, and of the remedies by which their effect was counteracted and cured.

The simple sap of the arbor toxicaria (according to Rumphius) is harmless, and requires the addition of ginger and several substances analogous to it, such as ledoory and lampoegang, to render it active and mortal. In so far it agrees with the antshar, which, in its simple state, is supposed to be inert, and before being used as a poison is subjected to a preparation which will be described after the history of the tree. The same effervescence and boiling, which occurs on the mixture of the substances added to the milky juice by the Javanese in Blambangan, has been observed in the preparation of the poison of Macassar, and in proportion to the violence of these effects the poison is supposed to be active.

A dissertation has been published by Chrisp. Aejmlaeus at Upsal, which contains the substance of the account of Rumphius; an extract from it is given in Dr. Duncan's Medic. Coment. for the year 1790, vol. ii. decad. v.

It appears from the account of Rumphius that this tree is also found on Borneo, Sumatra, and Bali.

Besides the true poison tree, the oopas of the eastern islands and the antshar of the Javanese, this island produces a shrub, which, as far as observations have hitherto been made, is peculiar to the same, and by a different mode of preparation, furnishes a poison far exceeding the oopas in violence. Its name is *tshettik*, and its specific description will succeed to that of the antshar. The genus has not yet been discovered or described.

Description of the Antshar.—The *antshar* belongs to the twenty-first class of Linnæus, the monoecia. The male and female flowers are produced in catkins (amenta) on the same branch, at no great distance from each other; the female flowers are in general above the male.

The characters of the genus are:

Male Flower.—*Calix,* consisting of several scales which are imbricate; *corol,* none; *stamens,* filaments many, very short, covered by the scales of the receptacle-anthers.

The receptacle on which the filaments are placed has a conical form, abrupt, somewhat rounded above.

Female Flower.—Catkins ovate; *calix,* consisting of a number of imbricate scales (generally more than in the male) containing one flower; *corol,* none; *pistil,* germ single, ovate erect; *styles,* two, long, slender, spreading; *stigmas,* simple acute; *seed-vessel,* an oblong drupe, covered with the calix; *seed,* an ovate nut with one cell.

Specific description.—The antshar is one of the largest trees in the forest of Java.—The stem is cylindrical, perpendicular, and rises completely naked to the height of 60, 70, or 80 feet. Near the surface of the ground it spreads obliquely, dividing into numerous broad appendages or wings, much like the *canarium commune* and several others of our large forest trees. It is covered with a whitish bark, slightly bursting in longitudinal furrows: near the ground this bark is, in old trees, more than half an inch thick, and, upon being wounded, yields plentifully the milky juice from which the celebrated poison is prepared. A puncture or incision being made in the tree, the juice or sap appears oozing out, of a yellowish colour; (somewhat frothy) from old trees, paler; and nearly white from young ones: when exposed to the air its surface becomes brown. The consistence very much resembles milk, only it is thicker, and viscid. This sap is contained in the true bark (or cortex) which, when punctured, yields a considerable quantity, so that in a short time a cup full may be collected from a large tree. The inner bark (or liber) is of a close fibrous texture, like that of the *morus papyrifera*, and when separated from the other bark, and cleansed from the adhering particles, resembles a coarse piece of linen. It has been worked into ropes, which are very strong, and the poorer class of people employ the inner bark of younger trees, which is more easily prepared, for the purpose of making a coarse stuff which they wear when working in the fields. But it requires much bruising, washing, and a long immersion in water before it can be used; and even when it appears completely purified, persons wearing this dress, on being exposed to the rain, are affected with an intolerable itching, which renders their flimsy covering almost insupportable.

It will appear from the account of the manner in which the poison is prepared, that the deleterious quality exists in the gum, a small portion of which still adhering to the bark, produces, when it becomes wet, this irritating effect; and it is singular, that this property of the prepared bark is known to the Javanese in all places where the tree grows, (for instance in various parts of the provinces of Bangil and Malang, and even at Onarang) while the preparation of a poison from its juice, which produces a mortal effect when introduced into the body by pointed weapons, is an exclusive art of the inhabitants of the eastern extremity of the island.

One of the Regents in the eastern districts informed me, that having many years ago prepared caps or bonnets from the inner bark of the antshar, which were stiffened in the usual manner with thick rice water, and handsomely painted, for the purpose of decorating his mantries, they all decidedly refused to wear them, asserting that it would cause their hair to fall out.

The stem of the antshar having arrived at the before-mentioned height, sends off a few stout branches, which spreading nearly horizontally with several irregular curves, divide into smaller branches and form a hemispherical, but not very regular crown. The external branches are short, have several unequal bends, and are covered with a brown bark.

The leaves are alternate, oblong, heart-shaped, somewhat narrower towards the base, entire, with a waving or undulated margin, which sometimes has a few irregular sinuosities. The longitudinal nerve divides the leaf somewhat obliquely, and the inferior division is generally the larger. The point is irregular; some are rounded at the end, others run off almost abruptly to a short point. The upper surface is shining and nearly smooth; some widely dispersed short villi are observed on it; the inferior surface is lightly rough, reticulated, and marked with oblique-parallel veins. The petiole is short. The flowers are produced towards the extremity of the outer branches, in a few scattered catkins; the common peduncle of the males is slender and long; that of the females is shorter.

Previous to the season of flowering, about the beginning of June, the tree sheds its leaves, which re-appear when the male flowers have completed the office of fecundation. It delights in a fertile and not very elevated soil, and is only found in the largest forests. I first met with it (the antshar) in the province of Poegar, on my way to Banjoowangee; in the province of Blambangan I visited four or five different trees, from which this description has been made, while two of them furnished the juice for the preparation of the oopas. The largest of these trees had, where the oblique appendages of the stem entered the ground, a diameter of at least ten feet; and where the regularly round and straight stem began, the extent of at least ten feet between the points of two opposite appendages at the surface of the ground, its diameter was full three feet. I have since found a very tall tree in Passooroowang, near the boundary of Malang, and very lately I have discovered several young trees in the forests of Japara, and one tree in the vicinity of Onarang. In all these places, though the inhabitants are unacquainted with the preparation and effect of the poison, they distinguish the tree by the name of Antshar. From the tree I found in the province of Passooroowang I collected some juice, which was nearly equal in its operation to that of Blambangan. One of the experiments to be related below was made with the oopas prepared by myself, after my return to the chief village. I had some difficulty in inducing the inhabitants to assist me in collecting the juice, as they feared a cutaneous eruption and inflammation, resembling, according to the account they gave of it, that produced by the ingas of this island, the *rhus vernix* of Japan, and the *rhus radicans* of North America: but they were only affected by a slight heat and itching of the eyes. In clearing the new grounds in the environs of Banjoowangie for cultivation, it is with much difficulty the inhabitants can be made to approach the tree, as they dread the cutaneous eruption which it is known to produce when newly cut down. But except when the tree is largely wounded, or when it is felled, by which a large portion of the juice is disengaged, the effluvia of which mixing with the atmosphere, affect the persons exposed to it with the symptoms just mentioned, the tree may be approached and ascended like the other common trees in the forests.

The antshar, like the trees in its neighbourhood, is on all sides surrounded by shrubs and plants; in no instance have I observed the ground naked or barren in its immediate circumference.

The largest tree I met with in Blambangan was so closely environed by the common trees and shrubs of the forest in which it grew, that it was with difficulty I could approach it. Several vines and climbing shrubs, in complete health and vigour, adhered to it and ascended to nearly half its height. And at the time I visited the tree and collected the juice, I was forcibly struck with the egregious misrepresentation of Foersch. Several young trees spontaneously sprung from seeds that had fallen from the parent, reminded me of a line in Darwin's Botanic Garden:—

> "Chained at his root two scion Demons dwell."

While in recalling his beautiful description of the oopas, my vicinity to the tree gave me reason to rejoice that it is founded on fiction. The wood of the antshar is white, *light and of a spongy appearance.*

Description of the Tshittik.—The fructification of the tshittik is still unknown; after all possible research in the district where it grows, I have not been able to find it in a flowering state.—It is a large winding shrub.

The root extends creeping to a considerable distance, parallel to the surface of the earth, sending off small fibres at different curves, while the main root strikes perpendicularly into the ground.

In large individuals it has a diameter of two or three inches; it is covered with a reddish-brown bark, containing a juice of the same colour, of a peculiar pungent, and somewhat nauseous odour. From this bark the poison is prepared.

The stem, which in general is shrubby, sometimes acquires the size of a small tree; it is very irregular in its ascent and distribution: having made several large bends near the surface of the earth it divides (at long intervals) into numerous branches, which attach themselves to the neighbouring objects and pursue a winding course, at no great distance from the ground and nearly parallel to it. In some instances the stem rises and ascends to the top of large trees; its form is completely cylindrical, and it is covered with a grey spotted bark.

The lesser branches arise from the stem in pairs (opposite) and are very long, slender, cylindrical, divergent, or spreading, and covered with a smooth grey shining bark; on these the leaves are placed opposite, in single pairs or on a common footstalk, pinnate in two or three pairs; they are egged, spear-shaped, entire, terminating in a long narrow point, completely smooth, and shining on the upper surface, with a few parallel veins beneath.—The petioles are short and somewhat curved. Towards their extremity the shoots produce cirrhi or tendrils, which appear without any regular distribution opposite to the leaflets; and some branches are entirely without them: they are about an inch long, slender, compressed, and spirally turned back (recurvati): at the end near their base a small stipula is found.

The tshettik grows only in close, shady, almost inaccessible forests, in a deep, black, fertile, vegetable mould. It is very rarely met with, even in the wildernesses of Blambangan.

1. *Preparation of the Antshar.*—This process was performed for me by an old Javanese, who was celebrated for his superior skill in preparing the poison. About eight ounces of the juice of the antshar, which had been collected the preceding evening in the usual manner, and preserved in the joint of a bamboo, was carefully strained into a bowl. The sap of the following substances, which had been finely grated and bruised, was carefully expressed and poured into it: viz. arum, *nampoo* (Javanese), kaemferia galanga, *kontshur*, amomum *bengley*, (a variety of zerumbed) common *onion* and *garlic*, of each about half a dram; the same quantity of finely powdered black pepper was then added, and the mixture stirred.

The preparer now took an entire fruit of the *capsicum fruticosum* or Guinea pepper, and having opened it, he carefully separated a single seed, and placed it on the fluid in the middle of the bowl.

The seed immediately began to reel round rapidly, now forming a regular circle, then darting towards the margin of the cup, with a perceptible commotion on the surface of the liquor, which continued about one minute. Being completely at rest, the same quantity of pepper was again added, and another seed of the capsicum laid on as before: a similar commotion took place in the fluid, but in a less degree, and the seed was carried round with diminished rapidity. The addition of the same quantity of pepper was repeated a third time, when a seed of the capsicum being carefully placed in the centre of the fluid, remained quiet, forming a regular circle about itself in the fluid, resembling the halo of the moon. This is considered as a sign that the preparation of the poison is complete.

The dried milk of the antshar having been preserved close a considerable time, can still be prepared and rendered active. A quantity which I had collected about two months before, was treated in the following manner by the same person who prepared the fresh juice. Being infused in as much hot water as was barely sufficient well to dissolve it, it was carefully stirred till all the particles soluble in water were taken up; a coagulum of resin remained undissolved, this was taken up and thrown away. The liquor was now treated with the spices above-mentioned, the pepper and the seed of the capsicum, in the same manner as the fresh juice. The same whirling motion occurred as above described on the seed being placed in the centre. Its activity will appear from one of the experiments to be related.

2. *Of the Tshettik.*—The bark of the root is carefully separated, and cleared of all the adherent earth; a proportionate quantity of

water is poured on, and it is boiled about an hour, when the fluid is carefully filtered through a white cloth. It is then exposed to the fire again, and boiled down to nearly the consistence of an extract; in this state it much resembles a thick syrup. The following spices having been prepared as above described, are added in the same proportion as to the antshar, viz. kaempferia galanga, (*kontshur,*) *soonty* &c. *Dshey*, for common *onion*, *garlic*, and *black pepper*.

The expressed juice of these is poured into the vessel, which is once more exposed to the fire a few minutes, when the preparation is complete. The oopas of both kinds must be preserved in very close vessels.

Experiments.—I. *With the Antshar.*

Exper. 1.—A dog of middling size was wounded in the muscles of the thigh with an arrow that had been immersed into the newly prepared oopas, and had been exposed to the air one night. In three minutes he seemed uneasy, he trembled and had occasional twitchings, his hair stood erect, he discharged the contents of his bowels. An attempt was made to oblige him to walk but he could with difficulty support himself. In eight minutes he began to tremble violently, the twitching continued, and his breathing was hasty. In 12 minutes he extended his tongue and licked his jaws; he soon made an attempt to vomit. In 13 minutes he had violent contractions of the abdominal and pectoral muscles, followed by vomiting of a yellowish fluid. In 15 minutes the vomiting recurred. In 16 minutes, almost unable to support himself, with violent contraction of the abdominal muscles. In 17 minutes he threw himself on the ground, his respiration was laborious, and he vomited a frothy matter. In 19 minutes violent retching, with interrupted discharge of a frothy substance from his stomach. In 21 minutes he had spasms of the pectoral and abdominal muscles, his breathing was very laborious, and the frothy vomiting continued. In 24 minutes in apparent agony, turning and twisting himself, rising up and lying down, throwing up froth. In 25 minutes he fell down suddenly, screamed, extended his extremities convulsed, discharged his excrement, the froth falling from his mouth. On the 26th minute he died.

Dissection.—The abdomen being opened about five minutes after death, a small quantity of a serous fluid was found in the cavity; the liver, intestines, and other viscera were natural.—In the stomach a yellowish frothy mucilage was found adhering to the internal coat, which was contracted into wrinkles.

In the thorax the lungs were of an elegant florid colour, and gorged with blood, the pulmonary vessels exhibiting through their coats a florid sanguinary fluid: on puncturing the ascending aörta the blood gushed out of a florid colour.

In the venæ cavæ the blood was of the usual dark hue, and on puncture flowed out forcibly. The muscles of the extremities were remarkably pale: on tracing the wound, it was found inflamed, and in two places along its course a small quantity of blood was found effused between the muscle and tendon.

Exper. 2.—A dog about four months old was pricked in the muscles of the thigh with the oopas that had been prepared from the juice I collected in Poegar; the poison had remained on the arrow about 48 hours. In three minutes he began to tremble, and the wounded limb shook more considerably; he soon began to droop, hung his head, and extending his tongue, licked his jaws. In four minutes he began to retch; on the eighth minute he vomited, with violent and painful contraction of the pectoral and abdominal muscles, which agitated his whole frame. In nine minutes he vomited again with convulsive violence; the secretion of saliva was much increased, he stretched out his fore-legs as if he could with difficulty support himself, his head hanging to the ground; his breathing was slow and laborious. In 11 minutes he threw up frothy matter, with violent contraction of the abdominal and pectoral muscles, and throwing himself on the ground, cried out violently. In 12 minutes the vomiting returned, he cried more violently, was seized with convulsions, extended his extremities, and on the 13th minute he died.

On *dissection* a small quantity of serum was found in the abdomen. The intestines were natural, the liver was much distended with blood, as also the vessels of the kidneys.

The stomach still contained some aliment.

In the thorax the lungs were of a beautiful crimson colour and the vessels strongly distended; on puncturing the aörta the blood bounded out forcibly of an elegant florid colour; collected in a cup it soon coagulated; from the venæ cavæ the blood also sprung out forcibly of a dark livid colour.

The vessels on the surface of the brain were more than naturally injected with blood; as were the longitudinal and frontal sinuses. The wound was as in the last instance.

Exper. 3.—An animal called gendoo by the Javanese (the lemur volans of Linnæus) was pricked in the cavity of the ear with a mixture of the simple unprepared fresh juice of antshar, with a little extract of tobacco. It felt the effects very soon, and during the first minutes it was very restless; on the fifth minute it became drooping. In 10 minutes it was convulsed, and soon became motionless and apparently insensible. On the 20th minute it died.

It must be remarked that this animal is uncommonly tenacious of life.

In attempting to kill it for the purpose of preparing and stuffing, it has more than once resisted a violent strangulation full 15 minutes.

Exper. 4.—A young lutra (welinsang of the Javanese) was punctured near the anus in the muscles of the abdomen, with the simple fresh juice of the antshar, mixed with a little extract of stramonium; very soon after the puncture the animal became restless, and holding it in my hand, I could perceive convulsive twitchings of the muscles. In 15 minutes it began to retch, had an increased flow of saliva, and extended the tongue: the abdominal muscles acted violently, and at intervals were strongly contracted about the pelvis. In 20 minutes it was convulsed, very restless during the intervals, and made repeated efforts to vomit without throwing up any thing: the convulsions increased in frequency and violence until the 25th minute, when the animal died.

Exper. 5.—A small dog was wounded in the usual manner in the muscles of the thigh with the simple unprepared milk of the antshar. From the moment of the puncture he continued barking and screaming incessantly eight minutes; he now extended his tongue, licked his jaws, was seized with twitchings of the extremities and with contractions of the abdominal muscles, and discharged the contents of his bowels. On the 10th minute he sprung up suddenly and barked violently, but soon became exhausted and laid down quietly on the ground. On the 12th minute he fell prostrate, was convulsed, after which having remained apparently motionless one minute, the convulsions recurred with greater force. On the 14th minute he died.

On *dissection* all the vessels in the thorax were found excessively distended with blood.

In the abdomen the stomach was almost empty, but distended with air and its internal coat covered with froth. The vessels of the liver were gorged with blood.

Exper. 6.—A bird of the genus ardea, somewhat smaller than a fowl, was wounded in the muscles of the abdomen with a dart covered with the unprepared milk of the antshar. On the sixth minute after the puncture it died, without exhibiting much of the effects of the poison, having been held in the hand to prevent its escape.

Exper. 7.—A bird of the same genus (as employed in the last experiment) was wounded in the muscles of the inferior part of the wing, with the unprepared milk of the antshar, collected from a different tree in the province of Blambangan. In 15 minutes he threw up a yellow matter from his stomach and trembled. In 20 minutes he died, having previously been convulsed.

Exper. 8.—A mouse was punctured in the muscles of the fore-leg, near the articulation, with the prepared poison. He immediately shewed symptoms of uneasiness, running round rapidly, and soon began to breathe hastily. In five minutes his breathing was laborious and difficult, and on the sixth minute not being able to support himself, he lay down on his side. In eight minutes he was convulsed, and his breathing was slow and interrupted; the convulsions continued until the 10th minute, when he died.

Exper. 9.—This experiment was made with the sap of the antshar, which I collected near the village of Porrong in Passooroowang, and prepared according to the process I had seen at Banjoowangee, with the spices above mentioned. As its object is to shew the relative action of the poison collected in different parts of the island, (and as it generally agrees with the first and second experiments,) I shall only mention its chief stages. In one minute after the puncture the animal began to shiver, and his skin was contracted. In five minutes he extended his tongue and began to retch. In eight minutes he trembled violently. On the 21st minute he vomited. In 24 minutes, after repeated vomiting, his extremities were convulsed. On the 29th minute he died.

The appearances on dissection were exactly the same as those observed in the first and second experiments.

Exper. 10.—The simple unprepared juice of the antshar from the same tree (vide Experiment 9,) applied on a small dog in the usual manner, caused death on the 19th minute, with the symptoms that occurred in the other experiments.

Exper. 11.—A small monkey was wounded in the muscles of the thigh, with a dart covered with the prepared oopas from Banjoowangee. He was instantly affected by the poison, and in less than one minute lay prostrate on his side: on attempting to rise he shewed symptoms of drowsiness, which continued five minutes, when he began to retch. On the sixth minute he vomited and discharged the contents of his rectum. He was soon seized with convulsions, and on the seventh minute he died. The same appearances were remarked on dissection as in the former experiments.

Exper. 12.—A cat was wounded with the same poison. In one minute the breathing became quick. In seven minutes the saliva flowed in drops from the tongue. In nine minutes she vomited a white frothy matter, and appeared in agony. On the 11th minute she threw up an excremental matter. In 14 minutes she discharged the contents of the bladder and rectum involuntarily. In 15 minutes she died convulsed.

Exper. 13.—The following experiment was made on the animal of the ox tribe, in common domestic use in Java, called korbow by the Javanese, and buffalo by the Europeans: the subject was full grown, and in perfect health and vigour. Having been well secured, he was wounded by a dart somewhat larger than those used in the other experiments, covered with the oopas from Blambangan (applied about 24 hours before) in the internal muscles of the thigh, in an oblique manner, the skin having been previously divided to admit the weapon freely.

The animal being in some degree loosened, about one minute after the puncture the dart was extricated: I suppose that about six grains of the poison adhered to the wound. On the 10th minute the respiration was somewhat increased and heavy. In 20 minutes he had a copious discharge from his intestines, a watery fluid flowed from his nostrils, and he showed some symptoms of drowsiness. In 30 minutes he had an increased flow of saliva which dropped from his mouth, he extended his tongue and licked his jaws; his respiration became more laborious; his pectoral muscles acted with violence, and the abdominal muscles were strongly contracted above the pelvis. His motions were slow and difficult. His muscular exertions were much diminished, and he exhibited great fatigue accompanied by restlessness: all these symptoms gradually increased until the 60th minute. His hair stood erect: unable to support himself, he lay down: he had contractions of the extremities: the abdominal and pectoral muscles were more violently convulsed and the respiration was more laborious. The restlessness rapidly increased; having risen with difficulty he quickly lay down again exhausted and panting; the flow of saliva from his mouth continuing. In 75 minutes he extended his tongue and made an attempt to vomit; his extremities trembled; he rose and threw himself down again suddenly, extending his head. On the 80th minute the saliva flowed in streams from his mouth mixed with froth: he retched violently, with excessive convulsive action of his pectoral muscles, but unable to vomit, he appeared in great agony. In 90 minutes he extended his head with strong convulsions, and trembled; the hair stood erect; he discharged the contents of his bowels; the breathing became more laborious, and the muscles of the abdomen and breast acted with excessive violence. The agony increasing, he rose a few seconds, but unable to support himself, fell down again. The 110th minute having made an attempt to rise, he fell down head foremost, with convulsions of the extremities and head; he groaned violently, the respiration was much impeded, and recurred at intervals of 15 seconds. On the 120th minute he lay in great agony, groaned, bellowed, and extended his tongue and extremities violently convulsed. In 125 minutes he was entirely exhausted: the breathing returned after long intervals. On the 130th minute he died convulsed.

15 minutes after the motions of life had ceased, I opened the cavities of the abdomen and breast. The stomach was immensely distended with air: the vessels of all the viscera of the abomen were as injected and distended with blood. In the thorax the lungs were of a vivid, florid, crimson colour, and the great vessels (the aörta, venæ cavæ, and the arteries and veins of the lungs) were gorged with blood.

A small puncture being made in the aörta, the blood bounded out in a stream of a beautiful crimson colour; from the venæ cavæ it flowed of a dark livid colour. In the large muscles of the pectus, which had been divided in the dissection, a trembling vibratory motion was observed full 20 minutes after the motions of life had ceased.

Exper. 14.—A fowl of middling size was punctured in the muscles of the thigh with a poisoned dart from Banjoowangee. During the first hour it was little affected by the wound. In about two hours it appeared drowsy, and had slight shiverings. It continued drooping and quiet till 24 hours after the puncture, when it died.

(To be continued.)

ARTICLE VI.

Tabular View of a Meteorological Journal kept at Lancaster. By J. Heaton.

1816.	THERMOMETER.					BAROMETER.					WEATHER.		WINDS.							
Months.	Highest.		Lowest.		Mean.	Highest.		Lowest.		Mean.	Snow or rainy days.	Fair days.	South.	South-west.	West.	North-west.	North.	North-east.	East.	South-east.
January	8	46°	29	2	37°	1	30·36	13	28·80	29·58	16	15	4	11	0	2	0	6	3	5
February	22	49	12	18	36	15	30·33	6	29·11	29·80	13	16	5	11	0	2	5	2	1	3
March	26	53	10	27	39	24	30·33	15	29·01	29·72	16	15	3	11	0	2	0	4	2	9
April	21	71	14	29	46	26	30·17	17	29·10	29·71	15	15	2	6	2	2	2	8	4	4
May	23	75	12	34	51	27	30·15	10	29·17	29·82	13	18	1	9	6	3	0	8	1	3
June	28	78	6	40	56	21	30·12	9	29·92	30·08	14	16	3	8	7	5	3	3	0	1
July	20	80	5	48	58	13	29·89	18	29·14	29·63	16	15	2	10	9	1	1	0	1	7
August	13	76	21	47	57	25	30·24	31	29·34	29·90	14	17	4	2	19	2	1	1	0	2
September	25	62	3	36	53	27	30·19	9	29·18	29·83	18	12	4	9	4	1	0	1	4	7
October	6	64	25	35	50	15	30·13	31	29·26	29·78	16	15	3	7	3	4	1	4	4	5
November	6	51	8	24	39	30	30·74	9	28·75	29·71	13	17	7	3	5	2	2	5	1	5
December	24	46	20	26	36	1	30·69	15	28·59	29·62	20	11	7	6	9	1	2	2	1	3
	July	80	Feb.	18	46½	Nov.	30·74	Dec.	28·59	29·765	177	182	45	93	64	27	17	44	22	54

Article VII.

Observations on Clouds, &c. By Mr. Johnson, Surgeon, of Lancaster.

(To Dr. Thomson.)

SIR,

The following remarks are submitted to your notice, in the hope that some of them may be deemed worthy of insertion in your *Annals of Philosophy*.

I. *On two Species of Cloud.*

1. In a serene sky, when there is no wind, a long and narrow band of white cloud may sometimes be observed stretching almost across the heavens, at a considerable elevation. One margin is thick and well defined; the other melts gradually off into a fine fleecy ragged edge; the thicker margin is always to windward with respect to the approaching rainy weather, of which this is one of the earliest and most certain harbingers. Each extremity appears to taper into an acute point. Probably this cloud floats very high in the atmosphere, and extends itself very far; in one instance it was noticed about the same time at Preston and Lancaster, its centre appearing vertical at both places, which are upwards of 20 miles distant from each other. I would term this a *lanceolate* cloud, although, as above described, it is, more correctly speaking, *ligulate*; but other varieties pass into an *elliptical*, an *oval*, and even a *triangular* form; all, however, are distinguished by a thick, well defined margin to windward, and a pointed extremity. The elliptical and oval varieties are well known hereabouts by the name of "Noah's Ark," either from their shape resembling the horizontal section of a canoe, or more figuratively because their appearance foretells rain and floods. The triangular form is only an incomplete variety, generally of a darker colour, accompanied by other clouds, by wind, a falling barometer, and other indications of approaching rain,

2. Near the horizon, between north and north-west, or about the magnetic pole, a very remarkable cloudy appearance sometimes presents itself. Its shape may be compared to a man's hand, the palm just emerging above the horizon, and long tufts expanding like fingers; these tufts, and of course the more dense parts of the cloud, are of a dark brown or black colour. Neither this, which may be termed the *plumose*, nor the lanceolate cloud just described, affects any particular time of the day or night; but I am not certain of having observed the former during winter, whilst the finest or most ligulate specimens of the latter have occurred towards the close of a frost. Both, however, seem worthy of notice as early harbingers of an impending change to bad weather, occurring at a

time when seafaring men, and others accustomed to notice atmospheric phenomena, will confidently anticipate weather of a different kind. On the 17th of July last there was a plumose cloud at seven in the evening; the barometer began to fall soon afterwards; rain came on in the night; and we had not a fair day for a week. On Aug. 14 there was a similar appearance at four in the morning; rain came on in the course of the day, and continued with only occasional interruptions for four days.

II. *On the Nomenclature of Clouds.*

It puzzles me to conjecture what benefit could accrue to meteorology from a relapse into the barbarous language of our Teutonic ancestors. It has hitherto been generally assigned as one good reason for cultivating an acquaintance with the languages of Greece and Rome that they furnish the materials out of which scientific words may be so coined as to pass current in all countries. But if the terminology of every branch of knowledge is to be formed out of a dialect peculiar to itself, the labourers in the temple of science may be as numerous, as zealous, and as enterprising, as those of the Tower of Babel, but their toil will be rendered equally abortive by a confusion of tongues.

III. *On Milk.*

Professor Berzelius has committed some flagrant mistakes in his statement of the constituents of milk,* which has been followed by Dr. Henry,† and (as I suppose from his reference) by yourself. You will observe that the proportion of sugar is to the cheese as 35 to 28. Again, in cream there is said to be nearly as much sugar as butter. The proportion of water is excessively too great. Indeed, the whole is at first sight so very absurd, that the mistake would hardly seem to need pointing out. However, it is more easily discovered than corrected; yet a good standard of milk is much to be desired. If an index expurgatorius of chemistry should ever be published, I know of no article which would better deserve attention than this very useful animal product.

IV.

Inclosed is a tabular view of results (see Art. VI.) obtained from a meteorological journal kept at Lancaster by my friend Mr. John Heaton. The observations were made twice a day, about nine o'clock, with great correctness and regularity on good instruments from Mr. Newman. Mr. Heaton has not noted the quantity of rain; but this has been done by two other observers, as you will learn from the inclosed paragraphs taken from the Lancaster Gazette of the 4th inst. The fact that less rain has fallen during 1816 than on an average of eight preceding years, has also been noticed by Mr. Harrison, of Kendal:—"The total amounted to somewhat less than 48 inches. This quantity falls short of the mean annual average of the last eight years, 1816 included, by two inches." (Kendal Chronicle, Jan. 11, 1817.)

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* Med. Chir. Trans. iii. 272, et seq.

† Henry's Elements of Chemistry, 17th edit. ii. 338.

I am, Sir, your obedient humble servant,

Lancaster, Jan. 18, 1817. C. JOHNSON, Surgeon.

From the Lancaster Gazette, Jan. 4, 1817.

The depth of rain which fell at Lancaster in 1816 was 37·47 inches; which is something short of an average quantity, as appears by the annexed statement, from which it is also evident that the cause of the unusual backwardness of the late harvest was not a superabundance of rain.

	Inches.
January	3·02
February	1·89
March	2·60
April	1·91
May	2·72
June	2·42
July	3·96
August	2·32
September	5·51
October	3·32
November	3·22
December	4·58
	37·47

	Inches.
Depth of rain in 1809	45·46
1810	31·81
1811	41·75
1812	37·67
1813	33·15
1814	32·39
1815	44·05
7)	266·28
Average of seven years	38·04

We have received from another correspondent the following table of the depth of rain fallen in this town last year. Our readers will perceive that the two accounts differ materially in several of the months, as well as in the total quantity.

	In.	64ths.
January	2	54
February	1	43
March	2	38
April	1	50
May	3	34
June	2	16
July	2	29
August	2	50
September	4	54
October	4	48
November	1	56
December	4	06
	34	61
Rain in 1815	43	35
Less in 1816	8	38

Article VIII.

On the Mineralogical Description, and Mineral Map of the County of Perth, which was in the Contemplation of Lord Gray and others, who proposed to subscribe thereto, in 1814. By John Farey, sen.

(To Dr. Thomson.)

SIR,

The plan which Professor Jameson drew up, at the request of Lord Gray, in 1814, for a mineralogical *description* of the county of Perth, in which no mention of a mineral *map* occurs, having been inserted in the *Annals of Philosophy*, vol. vii. and referred to, with marks of your commendation, *Annals*, ix. 82, I presume to beg the favour of you to give an early place in the *Annals* to a copy of the answer which I returned to the application received through a mutual friend of his Lordship and myself, Mr. Atkinson, of Bentinck-street, for making a mineral map, &c. of that county; to which copy I beg the liberty of now adding three short notes: and am, Sir, your very obedient humble servant,

Howland-street, Jan. 8, 1817. John Farey, sen.

(Copy.)

DEAR SIR, *London, July* 11, 1814.

"I have considered the subject of the proposed mineral survey of the county of Perth, on which you mentioned my Lord Gray's application, through you, and beg you will inform his Lordship that the survey of a district on the north-west of Sheffield,* (where it would have been of great importance to have found lime-stone) on which I have been engaged, has hitherto prevented my writing.

"I have crossed the south-eastern part of the county of Perth, and judge, from its appearance, that I should be able to trace every individual rock or useful stratum, in all that district, without any difficulty, and lay down its stratification upon a map, which would contain much useful and curious information.

"The remaining and larger district of the county, to the north-west, I am too little acquainted with, from the reports of others, to judge of the facility with which the same could be done there, or of the value of such a map of the mountain district, when made, except as to the situation of its lime-stone rocks.

"Neither am I acquainted with the extent, in square miles, of the two mineral divisions that might be made of Perthshire, a above, or with the state of the best large map of that county. If a thoroughly good modern map does not already exist, it will be absolutely necessary, for the purposes of a useful mineral survey, that the county map should be revised and filled up, at the time of examining and tracing the strata; since the positions of the strata are found intimately connected with the streams of water, even to the most minute ones, and the abrupt or steeper parts of the surface, occasioned by the edges of strata, are alike essential for tracing and explaining the internal structure of the district.

"For preserving and communicating the importantly useful knowledge of the stratification and mineral works (as mines, collieries, quarries, &c.), it is essential that *every object* on the surface should be represented in the map, in order to have a sufficient number of known points for exactly describing the situations of the several mineral objects.

"My want of information on the several points above mentioned makes it impossible for me to attempt any thing like an estimate of the expense of the proposed survey.

"The art of mineral surveying is at present so much in its infancy that the public in general are not sufficiently acquainted with its value to give the same extended encouragement to its professors as they do to other arts of longer established importance. On this account a surveyor can expect *no profit* from *the publication* † of large mineral maps, but in prudence must look for real employers before commencing such works.

"The great expense of publishing such surveys as I am in the habit of making induces me to take the liberty of suggesting a mode of preserving and diffusing the information; this is, that a Committee of a small number of the subscribers should be authorised to direct the survey, by employing one or more surveyors to proceed first with such parts as they may deem most useful and important; and that from time to time the work should be copied off from the surveyor's rough drafts upon a map of the county (by this means corrected fully, as the work proceeds, which may be deposited in Perth, or elsewhere), for the inspection and use of the subscribers, together with the specimens, and copies of the surveyor's notes and memorandums thereon.

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* See Phil. Mag. xlv. 161, since published.

† This alludes to a request that the estimate above alluded to might be made on the consideration of the mineral surveyor being at liberty to publish the map he should make.

"It would give me great pleasure to be employed by the subscribers to such a survey, on engagements for two or three months certain, at proper seasons; my charge for which would be two guineas * per day, and the repayment of my actual travelling and other expenses; which last, if the subscribers prefer, they might commute at one guinea per day, with some extra allowance for coach-hire, in the long journeys from and to London.

"I am, dear Sir, your obedient humble servant,

"*William Atkinson, Esq.* "J. FAREY, sen."

ARTICLE IX.

ANALYSES OF BOOKS.

I. *Prodrome d'une nouvelle Distribution systématique du Règne Animal.* Par H. de Blainville.

In the last number of our *Annals,* we promised to lay before our readers some account of the system of the classification of animals proposed by that learned and indefatigable zootomist, Professor Blainville; but as it is our intention to compare it with the systems of Professors Cuvier and De Lamarck, and to make some general observations on each, we must state the ideas of the former naturalist as briefly as possible.

His first notions on the subject were given in his public lectures in 1810; since which time he has devoted himself to the examination of the internal structure of organized beings, on which his classification and general distribution is founded. But what particularly demands our admiration is, that he has found external characters by which to distinguish each group. His next object is, to endeavour to establish a rational nomenclature, that will at the same time point out the most striking characters and situation of each group in his system; but he has carried this no further at present than to designate the primary divisions. We shall make remarks as we proceed, but shall first give a tabular view of his general classification, or distribution into—1. Sub-kingdoms. 2. Types. 3. Subtypes and classes.

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* Almost immediately after this period Mr. F. raised his charge to all new employers to three guineas per day, and expenses.

Sub-kingdom I.—Artiomophes.

(Animals whose organs are similar on each side.)

			Classes.
Type I. Vertebrose.	Sub-type 1. Viviparous.		1. *Pilifères*
	Sub-type 2. Oviparous.	with feathers	2. *Pennifères*
		with scales	3. *Squamifères*
		with naked skins	4. *Nudipellifères*
		with branchiæ	5. *Branchifères*
Type II. Avertebrose.	Sub-type 1. Not articulose.	head distinct	6. *Cephalofores*
		head none	7. *Acephaphores*
	Sub-type 2. Sub-articulose.	without legs	8. *Polyplaxiphores*
		with legs	9. *Cirrhipodes*
	Sub-type 3. Articulose.	legs six	10. *Hexapodes*
		legs eight	11. *Octopodes*
		legs ten	12. *Decapodes*
		legs various	13. *Heteropodes*
		legs fourteen	14. *Tetradecapodes*
		legs many	15. *Myriapodes*
		legs not articulated	16. *Setipodes*
		legs none	17. *Apodes*

Sub-kingdom II.—Actinomorphes.

(Animals with their bodies radiated.)

Sub-type 1. Sub-articulate	18. *Annulaires*
Sub-type 2.	19. *Echinodermaires*
	20. *Arachnodermaires*
	21. *Actiniaires*
	22. *Polypiaires*
	23. *Zoophytaires*

Sub-kingdom III.—Heteromorphes.

(Animals without regular form)	24. *Spongiaires*
	25. *Agastraires*

Dr. Blainville considers it erroneous to suppose that the vertical movement of the jaws is peculiar to vertebrose animals. He asserts that certain mollusca have the same motion in these parts; and he observes that the lower lip of insects (which is, in fact, formed of the coalesced exterior maxillæ) has only a vertical motion. He thinks, too, that the sepiæ from the disposition of their tentacules, resemble the polypiaires. He remarks that the nervous system of vertebrose animals, or rather the most developed part of it, is entirely surrounded by vertebræ. Thus the base of the brain appears to him to be no more than a continuation of the spinal marrow,* and the lower bones of the head to be coalesced vertebræ.

Class I.—*Mammalia, Pilifères, or Mastozoaires.*

These animals he distributes into two sub-classes: 1. *Monodelphés.* 2. *Didelphes.* The first he divides into six orders; the second contains but one. The cetaceous animals he has placed in the same order with the *édentés*; but observes that they should rather be considered as constituting an order by themselves. The genera *echidra* and *ornithorhynchus* he has placed with the *didelphes*, but he thinks they may probably form a distinct sub-class.

Class II.—*Aves, Birds, Pennifères, or Ornithozoaires.*

The classification of these animals he founds in the form of the sternum and its appendices; but here too he has shown us external characters by which his orders and families may be distinguished. The first order, *prehensores*, contains the parrots; the second, *raphores*, the birds of prey; the third, *scausores*, the climbing birds; the fourth, *saltatores*, the passarine birds; the fifth, *giratores*, the columbine birds; the sixth, the *gradatores*, or galinaceous birds; the seventh, *cursores*, or ostrich-like birds; the eighth, *grallatores*, or waders and divers; the ninth, *natatores*, the web-footed birds.

This disposition not only appears to be perfectly natural, but is at the same time superior in every point of view to any system of birds that has hitherto been proposed.

Class III.—*Squammiferes, or scaled Reptiles.*

The disposition of this and the following class is founded on the consideration of the skull, and was made public in Dr. Blainville's lectures delivered before the Faculté des Sciences as long ago as 1812. This class he divides into three orders—1. *Cheloniens*, or tortoises. 2. *Emydo-sauriens*, or crocodiles. 3. *Bisperiens*: sub-order, 1. *Sauriens*, or lizards with scales; 2. *Ophydiens*, or serpents.

Class IV.—*Nudipellifères, or naked Reptiles.*

Order, 1. *Batraciens*, or frogs. 2. *Pseudo-sauriens*, or salamanders. 3. *Amphibiens*, or protei and sirenes. 4. *Pseudo-ophydiens*, or coeciliæ.

Class V.—*Pisces, Fishes, or Branchifères.*

These animals he distributes into two sub-classes, from the implantation of their teeth, which he believes to be a character not hitherto employed by any other naturalist. These divisions, however, are not new, although distinguished by new characters. The first of these, *dermodontes*, contains the cartilaginous fishes, which in his opinion constitute four orders: 1. *Cyclostomes.* 2. *Selaques,* or rays and sharks. 3. *Esturgeons.* 4. *Polyodontes.* The second sub-class he divides into two tribes: 1. *Crustodermes,* or *branchiostèges.* 2. *Squammodermes,* or fishes properly so called, which have smaller scales than that of the first tribe; and these he distributes into order, 1. *Tetrapodes.* 2. *Dipodes.* 3. *Apodes.*

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* This fact was likewise inferred by Dr. Leach, who was led to the conclusion in consequence of the splendid anatomical discoveries of Dr. Spurzheim on the structure of the brain.

On the order *sélaques* he has written a very good monograph, with characters of the genera and their sub-divisions, with an enumeration of the species. The sub-genera in this essay are named on the principle of his nomenclature mentioned above.

Class VI. *Cephalopheres, or Cephalopodous and Gasteropodous Mollusca.*

As we shall in a future number give a detailed account of his arrangement of this and the three following classes, which he has written copiously in other parts of the Bulletin des Sciences, we shall at present merely give the names of his orders. A. With the shell symmetrical. Order, 1. *Cryptodibranches.* 2. *Pteradibranches.* 3. *Polybranches.* 4. *Cyclobranches.* 5. *Inferobranches.* 6. *Nucleobranches.* 7. *Cervicobranches.* B. Shell not symmetrical. Order, 8. *Chismobranches.* 9. *Pulmobranches.* 10. *Syphonobranches.* 11. *Monopleurobranches.*

Class VII. *Acephalopheres, or Acephalous Mollusca.*

Order, 1. *Palliobranches.* 2. *Lamellibranches.* 3. *Syphonobranches.* * Fixed: *a. Simple*; *b. Aggregated.* ** Free: *a. Simple*; *b. Aggregated.*

Class VIII. *Polyplaxiphores, or Chitones.*

Class IX. *Cirrhipodes, or Barnacles.*

Class X. *Hexapodes, or Insectes, properly so called.*

Sub-class, 1. *Tetraptères.* Order, 1. *Lepidoptères.* 2. *Coleoptères.* 3. *Orthoptères.* 4. *Hemiptères.* 5. *Neuroptères.* 6. *Hymenoptères.* Sub-class, 2. *Diptères.* Sub-class, 3. *Aptères.*

Class XI. *Octopodes, Arachnides, or Spiders.*

Class XII. *Decapodes, or Crustacea.*

Sub-class, 1. *Aceres.* Sub-class, 2. *Tetraceres.* A. *Thoraciques.* Order, 1. *Brachyceres.* 2. *Macroures.* B. *Athoraciques.*

Class XIII. *Heteropoda.*

Sub-class, 1. *Brachiopoda.* 2. *Squillares.* The first of these sub-classes contains a part of the *entomoertracea* of Müller. The second, the genus *squilla* of authors.

Class XIV. *Tétradécapodes.*

Sub-class, 1. *Tetracera.* Order, 1. *Crevettines.* 2. *Aselles.* 3. *Cloportes.* Sub-class, 2. *Epizores.* The first contains the *gammari* and *onisci* of the older authors; the second sub-class, the *lernææ*, *caligi*, &c.

Class XV. *Myriapoda*,

Contains the same animals included under this head by Dr. Leach.

Class XVI. *Setipodes, or Annelides*,

Comprehends the *lumbrici*, and other worms with setiform feet.

Class XVII. *Apodes*.

He is doubtful whether these animals, which he divides into subclass, 1. *Sang-sues*, or leeches; 2. *Entozaires*, or intestinal worms; belong to his first or second sub-kingdom.

Class XVIII. *Annulaires*.

Under this head are arranged the *siparculi* and kindred genera.

Class XIX. *Echinodermes*.

Order, 1. *Cylyndroïdes*, or holithariæ. 2. *Sphéroïdes*, or echini, and the genera divided from it. 3. *Stellérides*, or starfish.

Class XX. *Arachnodermaires, or Medusæ*.

Class XXI. *Actiniares, or Actiniæ*.

Class XXII. *Polypaëres*.

A. *Simple*. B. *Aggregated*. Order, 1. *Millépores*. 2. *Madrepores*. 3. *Rétépores*, or *eschares*. 4. *Cellepores*, or *cellaires*.

Class XXIII. *Zoophytaires, or Composed Polypes*.

Order, 1. *Tabulaires*. 2. *Pennatulaires*. 3. *Corollaires*.

These animals live in great societies, and are organically connected with each other.

Class XXIV. *Spongiaires, or Sponges*.

Class XXV. *Agastraires, or Infusaria*.

The agastraires comprehend only such of the infusaria as live and increase by absorption from without. Under the head of infusaria Dr. Blainville justly observes that several animals having very discordant structures have been included. The *corallinaires*, or genus *corallina* of authors, he has placed at the end of his paper, remarking, in a note, that in them he has never observed any sign of vitality, and that Mr. Brown regards them as appertaining to the vegetable kingdom.

We cannot conclude this very short sketch of Dr. Blainville's paper, without observing, that his system, although it does not in all parts accord with our views of the subject, contains abundance of valuable condensed matter. His arrangement of the birds, of the scaled and naked reptiles, and of the mollusca, is admirable.

We are not sufficiently acquainted with fishes to decide on the merits of that part of the subject; but it seems to be good, as far as we can judge. The disposition of the last eight classes is at least useful, if not partly natural: of the mammalia, with modifications, good; and of the entomozoaires, decidedly unnatural, although it must be admitted that the separation of the *setipodes* from the other *vermes* of authors is very much to be commended, and does infinite credit to the discernment of the author.

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II. *Mémoires sur les Animaux sans Vertèbres par* J. C. Savigny. *Seconde Partie. Mém.* 1—3. *Recherches Anatomiques sur les Ascidies simples et composées,* &c.

We have already mentioned the splendid anatomical observations of Savigny, whose valuable work on the *ascidiæ* has at length reached us.

The author has treated the subject under three heads of three memoirs. The first comprehends observations on gelatinous *alcyonia* with six simple tentacula, the animals of which have been confounded with *polypes** by all preceding writers; but their structure is totally distinct. Their bodies have two distinct cavities; they possess thoracic and abdominal viscera; they have an organ of generation; and the greater portion of them have beneath their external covering decided traces of a system of circulation. The rest of this memoir is occupied with details of their anatomical structure; but as this part is not susceptible of condensation, we must refer such of our readers as cultivate this branch of zoology to the work itself, the whole of which is written in the usual clear and comprehensive style of its author.

The second memoir contains observations of those *alcyonia* whose animals have two distinct openings, such as the *botrilli* and *pyrosomata,* which have no external tentacula.

The third memoir treats of the *ascidiæ* of authors, and contains several observations on the structure of those animals, as well as on the *composed ascidiæ* mentioned above.

The heart is by no means very evident in the *composed ascidiæ*; but on examining the various modifications presented in the different genera of *simple ascidiæ,* no doubt can remain in the minds of those naturalists who do not reject analogical proof. This organ is always situated close to the stomach, and the varieties of its form are infinite, and are fully detailed by Savigny.

All the *ascidiæ,* composed and simple, have a distinct nervous system, variously modified in the different genera, but always consisting of nerves and a large ganglion, considered as the brain, and which is brought into communication with the lateral ganglia when they exist. He considers them as constituting a class of the type MOLLUSCA. They have no distinct head; they are hermaphrodite. Their external covering is soft, and distinctly organized, with a branchial and anal opening. The organs of respiration occupy the whole or a part of the membranaceous cavity, and are attached to the internal surface of the external covering. Their mouth is placed at the bottom of the respiratory cavity between the two branchiæ, and is furnished with labial tentacula.

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* The polypes have a gelatinous body, with no other internal organ than a stomach with one opening, that serves the double purpose of mouth and anus.—LAMARCK.

This work is concluded with a systematic distribution of these animals into 14 genera, with descriptions of the species, elucidated by 24 highly finished plates, exhibiting their external and internal structure.

III. *Histoire Naturelle des Crustacées des Environs de Nice.* Par A. Risso. Ornée de Gravures. Paris, 1816.

In this work, which we have just received, the author has arranged the crustacea into two orders: 1. *Cryptobranches,* those with concealed organs of respiration. 2. *Gymnobranches,* those having naked or unprotected branchiæ.

The first order contains two sections: I. BRACHYARES, crustacea with short and naked tails. Family, 1. *Cancerides.* 2. *Oxyrinques.* II. MACROURES, crustacea with a long tail, furnished with ciliæ, hooks, or swimming plates. Family, 1. *Paguriens.* 2. *Langoustins.* 3. *Homardiens.*

The second order comprehends three sections: I. SQUILLINES, head distinct. Family, 1. *Squillares.* 2. *Crevettines.* II. TETRACERES, tail generally with foliaceous plates beneath. Family, 1. *Asellotes.* 2. *Cloportides.* III. ENTOMOSTRACES, with their head soldered to their bodies. Family, 1. *Clypéacés.* 2. *Ostracodes.*

He describes 132 species, which he has distributed into 53 genera, of which the following are the most interesting or new. Gen. *Anceus.* Thorax quadrate; mandibules very long, falciform and denticulated; tail furnished with three swimming plates. Sp. *Forticularius,* pl. 2, f. 10. This animal has long been known to British naturalists. It was first described by Montagu in the seventh volume of the Trans. Linn. Soc. 65, pl. 6, f. 2, under the name of *cancer maxillaris;* but neither Montagu nor Risso has been acquainted with its characters. The tail, in fact, has five swimming plates at its extremity; and, notwithstanding its sessile eyes, it is associated along with the *paguriens* by Risso, who informs us that he has so arranged it on account of its instinct, which resembles that of the genus hippa, and other paguriens with adactyle hands. It inhabits the interstices of madrepores. It is the *gnathia termitoides* of the Edinburgh Encyclopædia, vol. vii. p. 402.

A curious animal is described under the title *galathea glabra,* p. 72, and a fossil species, *galathea antiqua,* p. 73.

Janira,* a new and curious genus, whose antennæ are inserted in the same horizontal line, the interior ones with two setæ, the anterior pair of legs alone didactyle. Sp. 1, *Periculosa*, pl. 3, f. 1. The thorax is formed of transverse imbricated plates (or, as we should suppose appears to be so). He has placed it with the *homardiens*. It is a solitary species, and lives in deep water amongst the rocks. It is taken with difficulty; is said to swell like a bug, and to cause a pain in the stomach when eaten. The front is terminated by a spine, the wound of which is supposed to be very venomous. It is eaten by fish, in whose stomachs it has been found.

Thalassina littoralis, pl. 3, f. 2. This seems to be *cancer stellatus* of Montagu, and belongs to the genus *gebia* of Leach. The genus *thalassina* (Lutr.) is confined to the Indian Seas. *Gebia* is found in the Red and Mediterranean Seas, and European Ocean, burrowing beneath the ground at the bottom of the sea, and making long subterraneous passages, like a mole. Its body is covered with a soft crust, and is used in the Mediterranean as bait by the fishermen.

Nika, which is the same with the genus *processa* (Leach), is an animal of very singular conformation, one anterior leg being didactyle, the other monodactyle. Risso has described three species; one of which is sold at Nice during the whole year as food.

Egeon, a new genus allied to *palæmon* (the prawn), from which it differs in having no rostrum, and the anterior pair of legs monodactyle. He describes one species which is figured by Olivier. (Zool. Adriat. t. 3, f. 1. It is eatable, and has a worse flavour than the prawns.

Under the title *palæmon* he has described several species of a new genus; whilst under the generic name of *lysmata* he has described and figured one genuine *palæmon*.

Mysis Plumosus, p. 116. The animal described must be very interesting, but certainly does not belong to the genus in which he has placed it. Of the genus *phronima* he describes two species, distinguished by the third pair of legs, which are equal in the one, and unequal in the other.

In the same family with the last-mentioned genus he has given a new genus, named *typhis*, which he describes as having ten legs, but his figure exhibits parts for the attachment of 14 legs fitted for locomotion. He says that it is a very rare species, and is taken with difficulty. Pl. 2, f. 9.

Euphreus, a new genus 125, pl. 3, f. 7, with a depressed body. It is placed by Risso amongst crustacea with their bodies laterally compressed; it has probably some affinity with *cancer talpa mont*. Trans. Linn. Soc. ix. pl. 4, f. 6. (See Trans. Linn. Soc. ix. p. 372.)

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* This name he has substituted for *Calypso*, which he found to have been used before. In doing so he has fallen into a similar error. (See Trans. Linn. Soc. vol. xi. p. 373.

Talitrus. His generic character shows that he is not acquainted with the characters of the sexes which he has seen, as he describes the colour of the eggs of the female, and takes his generic character from the male only. His first species, to which the above remark is applicable, belongs to the genus *orchestia.* (Leach.)

Caprella punctata, 130, appears to be a new animal, and cannot belong to the genus to which he has referred it.

Idotea penicillata, 137, pl. 3, f. 10, is undoubtedly a new and very interesting species, but not referable to the genus idotea. It is more allied to the genus *anthura*, Trans. Linn. Soc. xi. 366.

Bopyrus, a parasitical genus, that insinuates itself beneath the sides of the thorax of the prawn, he has very properly referred to the family *asellotes.* It has been placed in various parts of the system of animals by different zoologists.

Ergyne cervicornis, 130, pl. 3, f. 12, a new and extraordinary genus, having four antennæ long and plumose. The body is depressed. The male is very small, and may always be found attached beneath the tail of the female.

Cyamus imbricatus is the same with *anthosoma smithii* (Leach), Suppl. Encycl, i. pl. 20. But this synonym could not have been determined, had we not seen a specimen sent by Risso to Latreille.

In the appendix, *autonomæa*, a new genus comprehending *cancer glaber*, Oliv. Zool. Adriat. pl. 3, f. 4, is given.

The system followed by this author is artificial, and his genera are for the most part artificial. It contains, however, abundance of valuable information on the economy of the species, which he has had an opportunity to examine in their native element, and will tend very materially to render our knowledge of this part of zoology more perfect and interesting.

ARTICLE X.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, Feb. 6, a paper by Mr. Edmond Davy, Professor of Chemistry to the Cork Institution, was read, on Fulminating Platinum. This new compound was prepared in the following manner:—Platinum reduced by exposing ammonio-muriate to a red heat was dissolved in nitro-muriatic acid, the solution was evaporated to dryness, re-dissolved in water, and the platinum precipitated in the state of sulphuret by passing a current of sulphureted hydrogen gas through the liquid. This sulphuret was digested in nitric acid till it was converted into sulphate of platinum. A little ammonia being poured into the liquid sulphate of platinum, a precipitate fell, which, being separated and washed, was put into a

Florence flask, along with a quantity of potash ley. Being boiled for some time, separated by the filter, washed, and dried, it was fulminating platinum.

This substance is a brown powder, varying in shade, and sometimes being very dark, according as the circumstances are varied in its preparation. It is specifically lighter than fulminating gold. It explodes violently when heated to the temperature of 400°, which is also the exploding temperature of fulminating gold. It does not explode by trituration or percussion. It is a non-conductor of electricity, which prevents it from exploding by the action of the galvanic battery. It indents a plate of metal when exploded on it in the same way as fulminating gold. When exploded between two plates, it acts most violently against the lower one. It dissolves in sulphuric acid, without giving out any gas. The solution is very dark coloured. Nitric and muriatic acids have but little action on it. Chlorine decomposes it, and converts it into muriate of ammonia and muriate of platinum. Ammoniacal gas has no action on it. When heated in muriatic acid gas, it is converted into muriate of ammonia and muriate of platinum. When exposed to the air, it absorbs a little moisture, but does not otherwise alter its properties.

On Thursday, Feb. 13, the remainder of Mr. Edmond Davy's paper on Fulminating Platinum was read. A great number of experiments were stated in order to determine the composition of fulminating platinum. 100 grains of the powder contain 73·75 grains of platinum. When the powder is treated with nitric acid, and heated cautiously, there remains a grey oxide of platinum, which Mr. Davy considers as new, and promises to describe soon. 100 grains of the fulminating powder left 82·5 grains of this grey oxide. Hence it follows that grey oxide of platinum is a compound of

Platinum	100
Oxygen	11·86

If it be considered as a protoxide, which is probable, it would indicate the weight of an atom of platinum to be 8·431. We may, therefore, reckon it as 8·5, without any considerable error. In order to determine the other constituents of fulminating platinum, he exploded small quantities of it in glass tubes over mercury. Ammonia was evolved, and water, and a quantity of azotic gas. From a careful comparison of the proportion of water and azotic gas emitted, the author concludes that the 17·5 grains wanting to make up 100 parts of the powder consist of 9 ammonia and 8·5 water. According to this statement, fulminating platinum is composed of

Grey oxide	82·5
Ammonia	9·0
Water	8·5
	100·0

Were we to suppose it a compound of two atoms grey oxide, one atom ammonia, and two atoms water, its constituents would be (reckoning an atom of grey oxide 9·5, an atom of ammonia 2·125, and an atom of water 1·125):—

Grey oxide	81·29
Ammonia	9·09
Water	9·62
	100·00

Now these proportions approach so nearly to those of Mr. Davy, that they serve very much to corroborate the accuracy of his analysis. The paper was terminated with a theory of the fulminating platinum. But as this theory nearly agrees with the previous theory of fulminating gold, as given by Bergman and Berthollet, I consider it as unnecessary to detail it here. The experiments in this paper appear to have been carefully made, and bear the stamp of precision.

On Thursday, Feb. 20, a paper by Mr. Pond, the Astronomer Royal, was read, on the Parallax of the fixed Stars. It is well known that Dr. Brinkley has for several years past been observing certain fixed stars with a circular instrument at the Dublin Observatory; that he has observed a sensible parallax in several of them amounting to about 2″, that this parallax has constantly appeared in every year's observations, and that it is too great to be ascribed to errors of observation. It was desirable that these observations should be confirmed by other astronomers. The circular instrument at Greenwich was considered as well adapted for the purpose: accordingly Mr. Pond made observations with it in 1812 and 1813; but he soon found that it would not answer the expected object, unless it could be wholly devoted to such observations. In consequence, he proposed at the last visitation that two ten-feet telescopes fitted with micrometers should be fixed to stone pillars, for the purpose of observing the parallax of the fixed stars, which proposal was approved of. Till these can be erected, two temporary telescopes have been fixed for making observations.

The object of the present communication was to state the result of the observations made in 1812 and 1813. The stars observed were α Aquilæ, α Lyræ, and α Cycni. The amount of the parallax did not exceed one-fourth of what Dr. Brinkley had observed; but it was constant, like that observed by Dr. Brinkley. Mr. Pond suspects that the difference is owing to some other cause than parallax; but he is far from being of opinion that the observations which he has already made are sufficient to decide the point. He hopes soon to be able to offer a new set of observations on this interesting subject.

LINNÆAN SOCIETY.

On Tuesday, Feb. 4, part of a paper by the late G. Anderson, Esq. F. L. S. was read, entitled, A Monograph of the Genus Pæonia. Linnæus at first confounded all the species of pæonia under the name *pæonia officinalis.* He afterwards added *pæonia tenuifolia,* and the *pæonia anomala* was admitted into his Mantissa. Since that time very little has been done by botanists to this genus, which is still involved in much confusion. The present monograph was owing to the zeal of Mr. Sabine, F. L. S. who collected into his garden all the varieties of pæonia to be found in Great Britain, to the number of more than 70. The descriptions were drawn up by Mr. Sabine and Mr. Anderson conjointly from living specimens.

All the species of pæonia belong to the northern hemisphere and to cold climates. None of them have been observed in America. They are all hardy enough to stand the winter in England. The species described are the following:—

1. *Montana.*—This constitutes the pride of the Chinese gardens, in which it has been cultivated above 1400 years. More than 200 varieties are known, and prized as much by the Chinese as the tulips are by the Dutch gardeners. This species is remarkable for the beauty and variety of its colours.

2. *Albiflora.*—Originally from Tartary. Introduced by seeds from Pallas. Different varieties are cultivated in England.

3. *Anomala.*—Originally from Siberia. Admitted by Linnæus in his Mantissa.

4. *Tenuifolia.*—Easily distinguished from the preceding species by its linear leaves. Admitted by Linnæus in the third edition of his Species Plantarum.

On Tuesday, Feb. 18, a paper by Capt. Marriott, of the Royal Navy, was read, giving a description of two shells. One a new species of mitra from the Mediterranean. The other, which he constituted a new genus, under the name of cyclosterma, was observed in a collection of shells chiefly West Indian.

At the same meeting, the remainder of Mr. Anderson's Monograph of the Genus Pæonia was read. Nine other species were described, making 13 in all; the principal of which were P. officinalis, corallina, humilis, arietina, peregrina, mollis, humilis.

WERNERIAN NATURAL HISTORY SOCIETY.

At the first meeting of this Society for the winter session (Nov. 23, 1816), Principal Baird communicated the copy of a letter from Lieut. Webb, dated Camp Fort, Peethora Gurh, April 2, 1816; in which that officer gives the altitudes of the principal snow peaks visible from Kumaon. He ascertained the height of 27 peaks in the Great Snowy Chain. The distances and bases were determined trigonometrically. The *lowest peak* measured was 15,733 feet above the level of the sea; the *highest peak,* 25,669 feet above the sea: 21 of the peaks were from 20,000 to 25,000 feet above the level of the sea!

At the same meeting Mr. Dacosta read a series of observations on the mineralogy of some districts in the north of Ireland, and detailed several analyses of the indurations observed where certain rocks meet together, from which it appeared that these indurated substances possessed a different chemical composition from the softer and supposed unaltered rocks.

At the next meeting, Dec. 7, Mr. Neill read an account of the capture of a beluga, or white whale, in the upper part of the Frith of Forth, in the month of June, 1815, and described its principal external characters and dimensions. The specimen, it appears, having fortunately fallen under the notice of a scientific gentleman at Alloa (Mr. Bald, civil engineer), was by him transmitted to Edinburgh. The animal was a male, and was nearly of full size. In length, it measured, in a straight line, 13 feet, 4 inches; and, where thickest, it was nearly nine feet in circumference. It was wholly of a rich white colour; and the singularity of its appearance attracted many hundreds of spectators for several days. While the animal was entire and fresh, a correct and beautiful drawing of it was made by Mr. Syme, painter to the Society. The external characters in general were found to agree with the descriptions given by Fabricius in the Fauna Groenlandica (delphinus albicans); by De la Cepede, in his Histoire des Cetacés (delphinapterus beluga); and by Sir Charles Giesecké, in the article *Greenland* in the Edinburgh Encyclopædia. Several healed wounds, the scars of which were still very obvious, indicated that this individual had probably been struggling among drift ice, and had in this way been carried far from his usual haunts. Mr. Pennant, in his writings, intimates a suspicion that the beluga occasionally visits our seas. It now appears that his conjecture was right: nor was this the first occasion on which the beluga has been seen on our shores. Mr. Neill mentioned that Col. Imrie, of the North British Staff, had, so long ago as the year 1793, examined two young and mottled belugas, which had been cast upon the beach near Thurso, in the Pentland Frith, and that the Colonel's description, taken on the spot, accorded generally with the accounts given by Crantz, Fabricius, and others.

At the meeting on Dec. 21, Dr. Barclay described the appearances which occurred on the dissection of the beluga above-mentioned. He arranged his observations under the following heads:—1. The integuments. 2. The tongue, alimentary canal, &c. 3. The organs of generation. 4. The os hyoides, larynx, trachea, and lungs. 5. The skeleton. 6. The organs of sense.

In examining the integuments, he found the rete mucosum about two-thirds of an inch in thickness, and evidently composed of two strata; the under stratum distinctly lamellated, with the edges of the laminæ at right angles to the stratum above, and which, on looking outward, from within, were observed separating and uniting again in waved lines, leaving interstices between them where they diverged, extending through the whole depth of the stratum. On viewing this stratum from the lateral aspect, the laminæ seemed obviously composed of fibres that were perpendicular to the stratum above and the cutis beneath.

The tongue was thick and short, restricted in its motions, and situated far back in the mouth. The œsophagus, when moderately inflated, was 13 inches in circumference. The stomachs were four; the first the largest, and which, with the last part of the œsophagus, was lined with a thick white coat, similar to that which is found lining the cardiac extremity in the stomach of a horse. The intestines were 28¼ yards in length, and without either a colon or cœcum: their circumference, when moderately inflated, between four and five inches. The spleen was attached to the first stomach, and not larger than an ordinary human spleen. The omentum lay chiefly between the stomachs. The liver was almost reduced to a jelly by putrefaction. The gall bladder was sought for, but not found. The pancreas was observed stretching from left to right, and every where blown up into large cells by extricated air. The kidneys, enveloped in a large and liquid mass of putrefaction, were inadvertently overlooked.

The testicles were situated near the anus, resting upon the sides of the intestine. The penis was without bone, and retracted into a sigmoid flexure by two muscles.

The heart presented nothing that was singular in its appearance. The aorta in circumference was 7½ inches where measured over the valves, 13 at the arch, and six where it rested on the vertebral column. The pulmonic artery over the valves was 11 inches in circumference, and immediately beyond the valves 9⅓.

The os hyoides consisted of four bones. The larynx of five cartilages, of which the epiglottis and the two arytenoids formed a tubes of some inches in length, that pointed towards the blow-holes. The trachea was composed of cartilaginous rings; but its branches, so far as they were traced in the lungs, of osseous rings. There were no front teeth in the upper jaw; and, like what occurs in some other animals with four stomachs, there was here a large branch sent off from the trachea before its division into two equal parts. The lungs on the right and left side were without lobes, and without adhesion. The skeleton of the head resembled that of a porpesse, but in this animal was very unlike the shape of the head when covered with the fat muscles and integuments. The vertebræ were easily distinguished into cervical, dorsal, lumbar, and caudal. The cervical were seven, the dorsal 11, the lumbar 13. There were no stemal cartilages, but stemal ribs, as in birds. The true ribs were six; the false five; and the last three of these articulated only with the extremities of the transverse processes. There was no pelvis nor sacral extremities; no clavicles, but large scapulæ. The other parts of the atlantal extremities, and a few of the distal caudal vertebræ, were removed with the integuments, and carried to the tanpit.

The brain was putrid; the spinal marrow proportionally small, the vertebral tube being chiefly filled up with a vascular plexus on each side, enclosed in an elastic cellular membrane. The eyes not so large as the human. The tongue, if an organ of taste, was so far back in the mouth that an object must have been half swallowed before it could be tasted. The blow-holes seemed rather organs of breathing than organs of smell; and nothing was found resembling the internal structure of nostrils. The organs of hearing, without and within, were sought for in vain.

ROYAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year 1815.

MATHEMATICAL PART.—*By M. le Chevalier Delambre, Perpetual Secretary.*

MEMOIRS APPROVED BY THE CLASS.

ANALYSIS.

(*Continued from* p. 159.)

Lectures on Analytical Mechanics given at the Polytechnic School, by M. de Prony. *Second Part, which treats of the Motions of solid Bodies*; *or, an Elementary Treatise of Dynamics.*

The first part of this work contained *Statics*, and we announced it in our notice for 1810. The second, which treats of motion, is divided in quite a similar manner: the laws of motion of a point of matter, of a system, or of a body of a form variable according to certain conditions, are successively explained in it, the whole being preceded by preliminary notions, in which the author has been at great care to establish in the most general analytical manner the fundamental principles of dynamics. After having explained every thing relating to time and its measure, he combines this species of quantity with linear extent, considering them as two indeterminate quantities, whose relations may be expressed by equations, according to which all the possible motions of a material point may be classed. The most simple of these relations gives *uniform motion*, from which proceeds velocity; and from this notion generalized he draws one of the fundamental equations applicable to every kind of motion. The second equation, which is equally general, flows equally from purely analytical considerations; so that we see an important theory established independent of the consideration of moving forces, by means of which we can, when certain phenomena are given by the fact, discover all those which are not already given.

The author applies this theory to the vertical motion of heavy bodies, whether in a vacuum, or through a resisting medium. He analyses successively the phenomena of motion which take place above the surface and in the interior of the earth, and shows how these accurate formulas may be deduced from those of Galileo.

He proceeds to the consideration of *force*. He distinguishes that whose effect is instantaneous from that which is subjected to the law of continuity. He demonstrates the laws which result from inertia, the reciprocal actions of bodies on each other, the theorem of the equilibrium of two material points which strike each other in opposite directions, and the formulas of this motion when equilibrium does not take place. These formulas are applicable to any degree whatever of elasticity, and he deduces from them with facility and as corollaries the cases of perfect hardness and perfect elasticity. He demonstrates occasionally some theorems respecting the *preservation of the motion of the centre of gravity*, the *preservation of living forces*, the loss of force in the case of *sudden change*, the principle of the *least action*. Among the examples of the theory of compound motion are found the determination of the angle of reflexions for any degree whatever of the elasticity of the body reflected, the examination of a case of motion which renders the difference between impingement and pressure sensible, and the properties of the machine of Atwood. He shows how the experiments should be made, and gives the requisite formulas for calculating them.

The second section treats of the motion of a material point, attending to the different conditions to which this motion may be subjected; those, for example, of moving either freely or along a given line or surface, or so as to satisfy certain conditions. Among the details which may be useful to those destined to cultivate the sciences will be distinguished the verification of the principle of *areas, the relations between the areas and the momenta.*

The author passes to the motion of heavy projectiles in a vacuum. He demonstrates analytically all the theorems of Galileo; and among the particular problems on that subject there occurs one whose solution is connected with the particular solutions of differential equations.

The fundamental equation for this motion does not suppose at first any hypothesis respecting the law of resistance. The author then introduces into it the law of the velocity, and deduces from it all the formulas necessary for those who have the management of artillery, treats of the resilience employing the simplifications allowed by the smallness of the angle of projection, and lays down the principal bases of the calculus to obtain the absolute numerical results for trajectories described in resisting mediums.

To this useful explanation one of great interest succeeds, that of the motions of the planets whether elliptical, parabolic or hyperbolic. This part of the work is so arranged as to constitute an elementary introduction to physical astronomy.

The motion of a heavy moveable body on a polygon serves as an introduction to the explanation of the general properties of a body moving in any curve whatever, fixed and continued. From this is deduced the theory of the simple pendulum, and the principal philosophical truths of which this instrument has occasioned the discovery or confirmation.

The cycloid and its properties, the discoveries of Huyghens, and the fine researches of Euler on tautochronic curves, then pass under review, and the author adds considerations on tautochronism in curves of double curvature.

The celebrated problem of the brachystochrone, solved at first in an elementary manner, and then examined in a general point of view, leads to one of the finest methods which mathematicians possess, the method of *variations.*

This section is terminated by the theory of the motion of a material point upon a given surface. The author verifies the principle of the least action in this kind of motion, determines the normal pressure of the surface, and applies this theory to the curious problem of the pendulum with *conical oscillations*; that is to say, which oscillates by turning round the vertical line drawn through the point of suspension.

The third section contains the general theory of motion, both of a system of material points, and of a continuous solid body, attending to their extent, and supposing their form invariable. It begins with the demonstration of the *general principle of motion,* which the author applies first to several questions previously resolved, and in particular to the machine of Atwood. He then brings into consideration the weight of the cord and the mass of the pulley, which he had at first neglected; and shows that the introduction of these elements would have occasioned no alteration in the consequences. He deduces from this principle the formulas of the motion of a body acted on by any forces whatever, and obliged to turn round a fixed axis. The formulas contain the remarkable expression of the *momentum of inertia*; and the author takes advantage of the recent extension given to this theory by M. J. Binet. The questions respecting the centre of oscillation and the compound pendulum naturally follow these researches. After having solved these questions, and demonstrated several curious properties in the compound pendulum, M. de Prony gives the theory of an apparatus which he contrived for giving the length of a seconds' pendulum when men of science were employed in fixing the basis of the decimal system of measures.

The problem of the centre of percussion, which in the 17th century had occasioned long discussions among philosophers, was solved only in particular cases. M. de Prony has supplied this defect by solving the problem with all the desirable generality.

The fine and difficult theory of the motion of rotation round a point was studied, and discoveries made in it, by Euler, Lagrange, and Laplace. M. Poisson has given an excellent account of it in his Treatise on Mechanics. M. de Prony, taking advantage of the labours of so many mathematicians, and of his own at different times, has endeavoured to make his explanations and developements as complete as possible. He treats of the case in which this motion takes place on a fixed plane, and takes as an example the problem of the top, which Euler appears to have first resolved.

The fourth section treats of the motion of bodies and systems of bodies whose form is variable.

This part of mechanics, notwithstanding the efforts of the greatest mathematicians, is far from possessing all the resources necessary for the solution of problems relative to systems, whether solid or fluid. From these considerations, and the impossibility of establishing a close connexion between the questions to be treated of in succession, M. de Prony formed the resolution of making this section consist of problems gradually increasing in difficulty, and connected with each other as much as the nature of the subject will permit.

The object of the first is the variation of the duration of the oscillations of a compound pendulum, when we displace a part of the mass of the pendulum. The author demonstrates the formulas which he had given in the Connaissance de Temps. He takes for one of his examples a problem which had occupied Euler and the Bernoulli. He examines the case of the oscillations of a heavy body obliged to move in a curve, and fixed to an immoveable body. He determines in this case the curve which possesses the property of tautochronism, and obtains a curious result relative to the cycloid. It is impossible for us to give to these details an extent proportional to their importance. We will satisfy ourselves with saying that the volume is terminated by an exposition which includes the demonstrations of all the great principles of mechanics. These different principles had been mentioned and verified several times in the body of the work; but had only been considered under particular points of view. The author thought proper to reserve the general demonstration till his readers were sufficiently prepared for it by anterior studies.

M. Charles Dupin, an officer of Maritime Engineers, Foreign Associate of the Institute of Naples, and Correspondent of the Class, presented the following works:—

PRINTED WORKS.

Memoir on the Re-establishment of the Marine Academy.—M. Dupin, who in the Ionian Isles exerted all his efforts in the organization of the first academy ever founded in these celebrated countries, endeavours in this new publication to hasten the revival of an institution of this kind applied to one of the most important branches of the public power and prosperity. He endeavours to prepare for this edifice more secure foundations, and better combined than those of the first creation. He points out and explains the means of spreading through the practice of our ports the theoretic perfections which originate in the capital, and to furnish in return to the scientific men who reside in this centre of civilization the maritime data to be procured only at sea and in the arsenals. He points out the investigations necessary for completing the maritime art in its most important branches, and shows the means of facilitating these labours, and rendering them fruitful.

Analysis of the Table of Military Naval Architecture in the 18*th and* 19*th Centuries.*

This writing explains very briefly the general plan of the work, and of the matters treated of in the first part only. This first part treats of the architecture of a vessel in general. The second treats of the comparative architecture of different kinds of vessels of war, from those of the first rate to the smallest boat. The author has merely presented to the Class the two volumes in quarto which constitute the first part. The one gives a picture of the structure of a complete vessel, its masts, sails, tackling, stowage, equipment. The other shows the methods of constructing a vessel from the first framing to the completion, its launching, its entry into dock, its careening, &c. Twenty plates of the largest atlas size accompany this first volume. The same number will accompany this. No known work on naval architecture takes in so vast a field.

MANUSCRIPT MEMOIRS.

M. Dupin has presented to the Class two memoirs, which are the sequel to his Developpemens de Geometrie, constituting the application of the mathematical theory of tracing routs.

In the first the author considers routs as intended to join a finite number of isolated points on surfaces of an arbitrary curvature.

In the second he supposes that the routs ought to serve for transport by infinitely small elements of masses which are continuous, linear, superficial, or solid. These are the problems which the labours of clearing and filling up offer in their greatest generality. M. Monge had treated the question on the supposition that the routs are constantly rectilinear. M. Dupin solves it on the supposition that the routs are subjected to follow the inflexions of any curved soil whatever.

A third memoir announced by the author contains the application of the preceding theories to the reflexion of pencils of light by a series of mirrors whose form and position are completely arbitrary.

Finally, M. Dupin has submitted to the Class the description which he drew up when he visited the arsenal at Rochefort of the machines contrived and executed in that arsenal by M. Hubert, a very distinguished naval engineer. The most remarkable of these machines, which are of great utility, and very ingenious, are the *moulins à scie et à drague.*

Voyage of Discovery to the Terra Australis, performed in the Corvettes le Geographe, le Naturaliste, la Goëlette, la Casuarina, during the Years 1800, 1801, 1802, 1803, *and* 1804, *under the Command of Captain N. Baudin — Navigation and Geography drawn up by M. Louis Freycinet, Captain of a Frigate, Correspondent of the Institute of France, Commander of the Casuarina in the Expedition: with an Atlas.* (Imprimerie Royale, 1815.)

"Without doubt this expedition was thwarted in a thousand ways. We may say, indeed, that no modern expedition was ever more disagreeable. But do not the results with which it has enriched the sciences render it so much the more honourable for those who have undertaken it, and pursued it with constancy?" This will not be a question with those who, like us, read the whole work of which we announce the first volume, who follow the whole details, and pay attention to the methods of observation, of calculation, and execution, which the author explains with a detail and fidelity well calculated to inspire confidence. He always indicates the sources of his information, the authors of the descriptions which he adopts, and those of the journals which he was obliged to strain, and at times to conciliate, when they exhibited different opinions.

An English navigator, to whose memory he pays a deserved homage, Capt. Flinders, has claimed the right of the first discovery relative to the south-west parts of New Holland; but in yielding this priority, which was never contested, either by Peron or himself, M. Freycinet observes justly that he could have had no knowledge of the labours of the English till after his return to France, and that the explanation of which he gives the history (and in which he took so active and honourable a part) was, notwithstanding, a labour of discovery. As to the names, it was impossible they could be the same, as there had been no communication between them. We know too well what is the custom of navigators in this respect, who often change the names imposed and published by their predecessors, which is without excuse. But Flinders and his countrymen did not do every thing; even after the French something remains to be done. When the examination is finished, when a complete account of these regions, at present so little known, shall be given, and of that difficult and dangerous navigation, then an impartial division may be made, and the names of the lands, islands, peninsulas, bays, and capes, given by those who first saw and described them, and who first fixed the longitudes and latitudes, and the true position of the coasts, may be adopted.

This volume is divided into four books. The first gives the itinerary and the general plan of the work. It is not in a voyage of this kind that we are exposed to that dryness for which the author wishes to apologise beforehand. The dangers of every kind which follow sufficiently support the interest and attention of the reader. In this respect the expedition of Baudin offers some characters which are peculiar to it. The part which the captain takes, to leave a boat sent by him to reconnoitre, his departure at the very instant when the galley Casuarina rejoined him, after an expedition for which he had only allowed 20 days, and the affectation with which he appeared to avoid her when she followed him, when she was in sight, and almost within reach, would lead one to think that Baudin sought for opportunities to get rid of some of his associates, who were not all equally his own choice, because he was far from taking an equal interest in all the parts of the vast enterprize which had been entrusted to him. But this captain sank under the fatigues of the expedition. He does not remain to give us an account of what may seem inexplicable in his conduct. Were we to condemn him without being heard, we should expose ourselves to the charge of that malevolence with which we stigmatize his memory.

The second book is devoted to geographical and nautical descriptions; that is, to the enumeration of the points perceived, and to the mass of observations which time and circumstances allowed them to collect on each coast. Here they treat of Van Dieman's Land and of Bass's Strait. We find a complete account of a great land which required two painful and difficult campaigns to explore; interesting discoveries on coasts each more than 300 leagues in extent; curious details respecting New South Wales, Port Jackson, several isles in the great Asiatic Archipelago, particularly Timor; observations of irregular and extraordinary tides, of the declinations and inclinations of the magnetic needle; not to speak of a great number of nautical and geographical facts, the noticing of which would lead us too far.

The third book gives an analysis of the charts. Here we may remark the memoir in which M. Boullanger gives examples of the calculations made to determine the rate of the time-pieces, and to correct the distance of the moon from the errors of the lunar tables; for these errors, how small soever they may be supposed, have still sensible effects upon the determinations, to which we wish to give the greatest possible precision.

The longitudes are fixed relatively to four principal points: 1. The fort Concordia, in the island of Timor. 2. The point Benilong, at Sidney Cove, near Port Jackson. 3. The observatory of Bernier, near a cape in the island of Crès. 4. The high point on the peninsula of Peron, in the bay of Sea Dogs. These longitudes were determined by east and west lunar distances for the first three, and east only for the fourth. The corrections for the errors of the tables were — 14′ 38″, — 13′ 28″, — 13′ 31″, + 6′ 57″. For the north-west harbour (channel of Entrecasteaux) and the point Maugé (isle Maria), the corrections were — 17′ 4″ and + 3′ 13″. The intermediate longitudes were calculated according to the corrected rate of the time-pieces.

Very good rules exist for laying down plans and sea charts; but to put them in practice the navigator must be master of the motions of his vessel. If he has not the chief command, he must be satisfied with taking the best advantage of the situation in which he happens to be, and endeavour by the number of his observations to compensate for the irregularity of the directions which he receives. The directions of the vessels were determined in the usual way, but with a care which it would be difficult to surpass. Watches were substituted for the sand-glasses, still most commonly employed, but the use of which should be entirely stopped. The errors of estimation have been diminished by all the known methods, and particularly by those furnished by astronomy and trigonometry. This may be judged of by the detailed example which the author gives of the method which he has followed, and which shows with what constancy he devoted himself to this disagreeable and troublesome labour.

The author then passes to the examination of the charts which compose his atlas. Their whole number is 32; but, besides the grand divisions, they exhibit particular plans of different remarkable points: so that the number of articles of which an account is to be given amount to 143. In engraving these upon copper, the methods pointed out and practised by Fleurieu have been followed with improvements upon them.

The fourth book is entitled, General Results. It contains— tables, in which we find the diurnal longitude and latitude of the vessels; the height of the barometer and thermometer, and the degrees of the hygrometer, the direction and strength of the wind, the state of the sky, the declination of the magnetic needle, the lunar and solar points, and different observations; tables of the positions determined during the whole expedition; tables of the tides; and tables of the currents observed near the different coasts. With regard to the typography, it is sufficient to observe that the work comes from the press of the Imprimerie Royale. Nothing has been neglected to render the execution of the atlas worthy of the enterprise which it is destined to perpetuate.

Travels of Ali Bey el Abbassi in Africa and Asia during the Years 1803, 4, 5, 6, *and* 7, *dedicated to the King. Three Volumes in* 8vo. *with an Atlas.*

"Ali Bey el Abassi (whose work we announce, and of whom the portrait is at the beginning of the first volume) is known in Asia and Africa as the son of Othman Bey, prince of the Abbassides.* Eager to acquire knowledge, and possessed of the most happy dispositions, he came when very young to study in Europe, where he acquired extensive information, which he afterwards applied to the practice of astronomy, geography, and natural history." To these lines, which we copy from the preface, we will add from the verbal account of the contents of this work, which we heard given to the Class, that the editor, M. B., is known very advantageously by several memoirs relative to different circumstances of the travels which he now publishes, and even by astronomical observations and geographical positions inserted in different volumes of the Connaissances des Temps. As to the person of the traveller, we see in the preface that doubts have often been started respecting his origin. To these suspicions he opposes the marks of esteem which he received from the Emperor of Morocco, even after attempts had been made to blacken him in the estimation of that prince: the way in which he was received at Tripoli, at Cairo, by the scherif of Mecca, by the Pasha of Acre, and by other great personages: but without entering in the least into the discussion, with which we have no concern, we shall merely point out briefly the principal new facts contained in the work. It cannot be denied that a Mussulman, known to be such, travelling in countries where every European is more or less suspected, is a very proper person to give us information respecting the customs, manners, and religious practices, of these people; ideas which from another would not inspire us with so much confidence. These details can be appreciated by a great number of readers. But what ought particularly to fix the attention of the Class are the astronomical, geographical, and meteorological observations, the direction of the routes followed by the caravans, as travellers are very seldom found capable of publishing such remarks. We see even that our Mussulman has been more than once thwarted in his scientific projects, either by the prejudices of those who surrounded him, or by other cares which constituted the principal object of his journey, and which to us would have been of little consequence, even if the author had given us that information respecting it which he reserves to another time.

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* Our readers are of course aware that the name and character of Ali Bey were assumed by an European.

The nature of the countries and their soil are described, as might have been expected from an attentive observer, who had not always an opportunity of stopping where he chose, and still less of making those excursions which he would have considered as proper. But he determines at least by the compass all the changes of direction in the road, and furnishes at various distances points whose longitude and latitude are determined by exact observations, to which he adds the declination of the needle.

The work is divided into three volumes, which form as many distinct parts.

The first contains the sojournment of Ali Bey in the kingdom of Morocco. It is terminated by new conjectures on the Atlantis: but after having considered this new system, compared with all the others which have been written on this subject, one is disposed to call to mind the observation of Aristotle, who, comparing this section of the Atlantis to that of the wall constructed by the Greeks to fortify themselves against the attacks of Hector, adds, that the *poet who contrived it has himself destroyed it so as not to leave a single vestige of it remaining*. These conjectures are followed by others which may have a better foundation. They relate to the existence of an inland sea in the centre of Africa, similar to the Caspian Sea. The traveller endeavours to strengthen his notion by calculations, and by joining together all the proofs which he was able to collect. But, notwithstanding his efforts, the conjecture is likely to remain long problematical.

The second volume contains his journey to Alexandria, and new and curious details respecting the island of Cyprus, the pilgrimage to Mecca, the description of the Temple, and of the Kaaba, or House of God. It is terminated by a notice of the Wehabis, and of their religious principles.

In the third volume we have an account of an unsuccessful attempt of the traveller to penetrate to Medina, his more successful journey to Jerusalem, the description of the Temple, the entrance into which is prohibited to Christians; and, lastly, very interesting information respecting Damascus and Constantinople.

The atlas contains 89 plates, among which the most remarkable are those relative to the temples of Mecca and Jerusalem, the map of the kingdom of Morocco, of Northern Africa, of the coast of Arabia on the Red Sea, and the itinerary of Ali Bey.

Algebraical and Geometrical Analytical Essays. By M. J. de Stainville, repetiteur-adjoint to the Polytechnic School. Mad. Veuve Courcier, 1815.

The object of the author is to develope some points of analysis which appeared to him susceptible of extension and simplification.

We know that algebraic calculation often leads by certain roads to results which it is impossible to doubt, but which astonish and confound the calculator till he succeeds in giving a natural explanation to the paradox. We have a very celebrated example in the irreducible case of cubic equations. All mathematicians have been eager to throw light upon it. After all that they have said, the reader will still see with pleasure the new explanation given by M. de Stainville. We shall say the same thing of the method which he has found for biquadratic equations, of all the details which he gives of the binomial theorem of Newton, of the new and simple method which he follows to obtain formulas that express the sines, co-sines, and tangents, of any number whatever of angles, to demonstrate the theorems of Cotes and Taylor, and of his different problems on the cycloid, the hyperbola, and the logarithmic. In all these developements the author makes a happy use of geometrical considerations, the advantage of which is to bring under a small number of principles truths that appear at first very complicated. The reader who is ignorant of them will acquire a knowledge of them with less labour: he who has long known them will perceive so much the better the intimate connexion which unites together all the parts of analytical science, and allows us to arrive at the same theorems by so many different ways.

ARTICLE XI.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Allophan.*

This name has been given by Stromeyer to a new mineral lately analysed by him, because it has very much the appearance of a copper salt.

Its colour is intermediate between sky-blue and verdigris-green. Fracture conchoidal. Lustre intermediate between vitreous and waxy. Translucent. Specific gravity from 1·852 to 1·889. It occurs in amorphous pieces in a marly rock. According to the analysis of Stromeyer, its constituents are—

Alumina	32·202
Silica	21·922
Lime	0·730
Sulphate of lime	0·517
Carbonate of copper	3·058
Hydrate of iron	0·270
Water	41·301
	100·000

II. *Silverkupferglanz.*

Professor Stromeyer has lately analysed a new ore, found by Professor Hausmann, in the Museum of the Academy.

Its colour is intermediate between lead-grey and iron-black, with a tint of copper-red. Its fracture is conchoidal, and its lustre metallic. It is mild, and has the specific gravity of 6·255.

According to Stromeyer's analysis, it is composed of

Silver	52·2722
Copper	30·4787
Iron	0·3331
Sulphur	15·7824

He considers it as a compound of

Sulphuret of silver	60·646
Sulphuret of copper	38·654
Sulphuret of iron	0·700
	100·000

III. *Salts of Strontian.*

Professor Stromeyer, of Gottingen, has lately made a very careful analysis of various salts containing strontian for their base. The following are the results which he obtained :—

1. *Carbonate of Strontian.*

Carbonic acid	29·687	42·2212
Strontian	70·313	100
	100	

Native carbonate of strontian from Argyleshire contains two per cent. of carbonate of lime.

2. *Sulphate of Strontian.*

Sulphuric acid	43	75·44
Strontian	57	100
	100	

3. *Nitrate of Strontian.*

Nitric acid	50·62	102·511
Strontian	49·38	100
	100	

4. *Muriate of Strontian.*

Muriatic acid	34·415	52·474
Strontian	65·585	100
	100	

5. *Phosphate of Strontian.*

Phosphoric acid	36·563	57·6417
Strontian	63·436	100

An atom of strontian, according to these experiments, weighs 6·5228.

IV. *Heights of different Places in France above the Level of the Sea.*

Plains of Beauce	175 metres
Paris	34
Chalons-sur-Marne	109
Lyons	175
Dijon	263
Orleans	203
Grenoble	307
Valence	111
Grange	64
Besançon	161
Nancy	253
Plains of Switzerland	428 to 487
Plains of Lombardy	78 to 126
Plains of Castile	585

(Ann. de Chim. et Phys. ii. 386.)

V. *Necessity of Food containing Azote for the Nourishment of Animals.*

It is a commonly received opinion that sugar, gum, oil, butter, and some other similar bodies, which contain no azote, constitute very nourishing articles of food. This opinion has been put to the test of experiment by M. Magendie. A dog was fed upon sugar and distilled water. He eat his food readily, and for some time retained his health and usual liveliness. But in about a fortnight he began to get lean, though his appetite continued good. His alvine excretions were scanty, but his urine was abundant. On the 21st day an ulcer began to appear in the centre of the cornea of each eye, which gradually increased, penetrated the cornea, and the humors of the eye ran out. The leanness continually increased, the animal lost its strength, and died on the 32d day. The dead body was found destitute of fat, and the muscles deprived of five-sixths of their usual volume. The urine was alkaline, and destitute of uric acid, like that of herbivorous animals. The bile also contained much picromel, like that of oxen. A second and a third dog, fed likewise upon sugar and water, shared a similar fate.

Two dogs fed upon olive oil and water died on the 36th day, with precisely the same phenomena, except the ulceration in the cornea, which did not take place.

Several dogs were fed with gum and water. Their fate was precisely the same.

A dog fed on butter died on the 36th day, with precisely the same phenomena; though on the 34th flesh was given him, and he was allowed to eat of it at pleasure. In him the ulceration in the cornea showed itself.

From these curious experiments of M. Magendie, it is obvious that none of these articles are capable of nourishing dogs. Hence we may infer, with the greatest probability, that they are incapable of nourishing man. (Ann. de Chim. et Phys. iii. 66.)

VI. *Temperature of the Sea.*

1. At the surface of the ocean, at a distance from land, the temperature of the water at noon is colder than that of the air. Its temperature at midnight is always higher than that of the air; so that twice every 24 hours the temperature of the sea and that of the air are the same.

2. The mean temperature of the surface of the sea at a distance from land is higher than that of the air.

3. The temperature of the sea, supposing no current, diminishes in proportion to its depth. (Peron, Ann. de Chim. et Phys. iii. 126.)

VII. *Sulphate of Barytes in Surrey.*

Some years ago many beautiful specimens of sulphate of barytes were found in the fuller's earth at Nutfield, in Surrey. They may be still seen, I presume, in Mr. Sowerby's museum; and are very remarkable for the brilliancy of their yellow colour. A specimen from Mr. Sowerby's collection has been recently analysed by Professor Stromeyer, of Gottingen, who found its composition as follows:—

Barytes	65·807
Sulphuric acid	33·874
Hydrate of iron	0·051
Colouring matter and water	0·053
	99·785
Loss	0·215
	100·000

The colouring matter he considers as only mechanically mixed with the sulphate. It is dissipated or destroyed by a moderate heat.

VIII. *Celestine from Dornburg, near Jena.*

This substance has been lately discovered at Dornburg, constituting a bed in marl. It is fibrous, and has a blue colour, similar to that from Pennsylvania. Its specific gravity is 3·9536. Stromeyer has analysed it, and found its constituents as follows:—

Strontian	56·397
Sulphuric acid	42·949
Lime	0·057
Oxide of iron	0·027
Clay	0·051
Bitumen and water	0·105
	99·582
Loss	0·418
	100·000

The lime, iron, and clay, in Stromeyer's opinion, come from the marl in which the celestine is found. The blue colour is owing to the bitumen, which is only mechanically mixed.

IX. *Vulpinite.*

This is a name given by some mineralogists to a mineral found at Vulpino, not far from Bergamo, in Lombardy. In Italy it is known by the name of *marmo bardiglio di Bergamo*. It was noticed in the Journal de Physique under the name of *pierre de Vulpino*. Haüy, relying on an analysis of Vauquelin, gave it the appellation of *chaux anhydro-sulphatée quartzifere*. Stromeyer has lately analyzed it, and found its constituents as follows:—

Lime	41·710
Sulphuric acid	57·966
Quartz	0·090
Water	0·072
	99·838
Loss	0·162
	100·000

Another specimen of coarse scaly vulpinite, which he received from Professor Pfaff, of Kiel, was composed of

Lime	41·398
Sulphuric acid	56·641
Quartz	0·260
Oxide of iron	0·033
Water	0·957
Loss	0·711
	100·000

It would appear, therefore, that the vulpinite is merely an anhydrous sulphate of lime.

X. *Discovery of the Existence of Cobalt in Meteoric Iron.*

The existence of nickel and chromium in meteoric stones has long been known; and an experiment of Klaproth (Beitrage, vi. 297) leads to the suspicion of the existence of cobalt in the same minerals. The existence of cobalt in the meteoric iron from the Cape of Good Hope has been ascertained by Professor Stromeyer, who lately analyzed a specimen of it sent him by Mr. Sowerby. He was unable to detect cobalt in the meteoric iron from Siberia and Bohemia; but he considers the methods at present used to separate cobalt from nickel as bad, and he has not yet discovered a better.

XI. *Death of Klaproth.*

On Jan. 1, 1817, Martin Henry von Klaproth, Professor of Chemistry at Berlin, and by far the most celebrated chemist in Germany, died at a very advanced age. He has been a distinguished writer for at least 40 years; for he published a set of chemical experiments on copal in the year 1776. Chemistry lies under greater obligations to him than to any other chemist of his time. He devoted himself entirely to analytical chemistry; and to him we are chiefly indebted for the knowledge which we at present possess of the mineral kingdom, and for the formulas employed to develope the constituents of minerals. His labours are consigned in six octavo volumes, under the title of Beiträge zur chemischen Kenntniss der Mineralkörper, the first volume of which was published in 1795, and the last in 1815. He was the discoverer of uranium, and he confirmed and completed the discovery of tellurium and titanium. He likewise discovered zirconia and mellitic acid.

XII. *Diabetic Urine.*

(To Dr. Thomson.)

SIR, *Bedford-square, Feb.*, 1817.

If you, or any of your correspondents, will in your next number treat on the most effectual method of analysing diabetic urine, it will much oblige,

Yours respectfully,

J. L.

* * *

I think my correspondent cannot do better than study the experiments of Nicolas in the Annales de Chimie, xliv. 32, those of Sorg in Gehlen's Journal, vi. 9, those of Dupuytren and Thenard in the Annales de Chimie, lix. 41. He will find it useful to peruse the experiments of Fourcroy and Vauquelin in the Annales de Chimie, xxxi. 48, and Berzelius's paper on animal fluids published in the *Annals of Philosophy*, ii. 19, &c. If he understands

Swedish, he cannot be referred to a better book than Berzelius's Djürkemien.—T.

XIII. *Query respecting Carmine.*

(To Dr. Thomson.)

SIR,

I shall feel myself much obliged if you, or any of your correspondents, will, through the medium of your *Annals of Philosophy*, favour me with the readiest method of preparing carmine, having ineffectually tried it by most of the methods recommended by Nicholson and Aikin.

Yours, with the most profound respect,

J. N. R. M.

XIV. *Aurora Borealis at Sunderland.*

(To Dr. Thomson.)

SIR,

On Saturday night, the 8th inst., about seven o'clock in the evening, during a strong gale from the north-west, which had continued five days, was observed the most beautiful aurora borealis, such as had not been seen here for upwards of 20 years. It began in single bright streamers in the north and north-west, which, gradually increasing, covered a large space of the hemisphere, and rushed about from place to place, with amazing velocity, much higher than the zenith, and had a fine tremulous motion. They illuminated the hemisphere as much as the moon usually does when eight or nine days from the change. About 11 o'clock part of the streamers appeared as if projected from a centre south of the zenith, and looked like the pillars of an immense amphitheatre, presenting the most brilliant spectacle that can be conceived, and seeming to be in a lower region of the atmosphere, and to descend and ascend in the air for several minutes. One of the streamers passed over *α* in the right shoulder of Orion, but neither increased nor diminished its splendour. About eight o'clock Venus was about 8° above the horizon, and displayed a very peculiar appearance; for her rays passed through a thin mist or cloud, probably electric, of a deep yellow tint. Her apparent magnitude seemed increased; and a halo was formed round her, as sometimes appears round the moon in moist weather; but the stars that were in that part of the hemisphere shone with their accustomed brilliancy.

This phenomenon, among English writers, is first described by Matthew of Westminster, who relates that in A. D. 555 there were certain appearances of lances seen in the air from the north to the west, or, to use his own words, "quasi species lancearum in aere visae sunt a septentrione usque ad occidentem." (P. 101.) And may not the following line in Virgil apply to this phenomenon;

> "—— Armorum sonitum toto Germania coelo
> Audiit." Virg. Georg. I. 474.

Should the above observations deserve a place in your philosophical journal, the insertion will much oblige,

Sir, your obedient servant,

Bishop-wearmouth, Feb. 10, 1817. **Robert Pensey.**

XV. *Formulas for estimating the Height of Mountains.*

(To Dr. Thomson.)

Dear Sir,

The perusal of Mr. Anderson's very ingenious paper on barometric altimetry in your last number, brought to my recollection a couple of formulæ which I composed in one of the latter months of 1815. Before sending them to you, I wished to ascertain if my discovery had been anticipated, but have had no opportunity of satisfying myself on that head. The formulæ of Sir G. Shuckburgh and Professor Leslie, I conclude, must be of general application; if so, they are quite distinct from mine, which are exclusively adapted to the use of observers in countries of moderate inequality, like our own island. At all events, they have the merit of utility and convenience, whatever be the fate of their claims of originality; and as, by every rule of probabilities, the present year ought to be favourable for making observations, you will perhaps deem this a favourable crisis for stimulating by discussion the spirit of curiosity which a long series of bad weather has torpified.

1. If T, t, represent the temperatures of the air, taken at the bottom and at the summit of the mountain, by a detached thermometer; and B, b, the heights of the mercury in the barometer in the same situations; the elevation, expressed in yards, will be equal to

$$\frac{20\,(B-b)}{B+b} \times (808 + T + t)$$

The result found from this theorem will not deviate from that obtained by logarithms, more than three or four feet in the height of Ben Nevis, estimated at 1450 yards.

For altitudes of about 3640 feet, it is precisely equivalent to the logarithmic method.

In lower elevations, the greatest error, about two or three feet, occurs at the height of 700 yards.

These errors are so minute, particularly when compared with those which will arise from physical sources, that the observer who preserves this formula in memory, or in his memorandum book, need not regret the occasional absence of logarithmic tables.

When the circumstances of the observation appear to admit of great accuracy, it will be proper to employ $b + \frac{b\,(T' - t')}{10000}$ instead of b, as a correction due to the diminished temperature of the mercury from T' to t', as shown by a thermometer attached to the instrument.

2. Another formula, very easy of computation, is the following:

$$\left(59 - \frac{B + b}{2}\right) \times 10\,(B - b) \times (1 \pm \cdot 0023\,T)$$

This is accurate when the mean height of the mercury is either 29 or 30 inches; and very nearly so for any elevation not exceeding 700 or 800 yards above the sea. Where the mean height is much less than 28½ inches, the error becomes progressively important.

The easiest way of correcting the logarithmic result for difference of atmospheric temperature is by means of the third factor in my second formula; in which T expresses the difference between the mean temperature and 31°. The first formula includes this correction; and on that account may be compared in point of facility, as well as correctness, with any method of computation yet known.

I am yours, most respectfully,

Bath, Feb. 13, 1817. W. G. Horner.

P.S. Does not the excessive dryness of the atmosphere so eloquently described by Col. Beaufoy sufficiently explain the unexpected result of his experiment with the burning lens on the summit of Mont Blanc?

XVI. *Improvement suggested in the new Blow-pipe of Mr. Brooke.*

(To Dr. Thomson.)

DEAR SIR, *Feb.* 6, 1817.

Being a constant reader of your *Annals of Philosophy*, and observing several proposals for improving the gaseous blow-pipe, but thinking none of them perfectly safe, I venture the following ideas. Suppose there was a circular hole in the bottom made as large as the reservoir would permit, with a thin piece of tin or lead soldered over it, this secured over a hole in a table, under which is placed a tub of sand. If an explosion should take place, the piece soldered on will be driven into the tub. This, in addition to Professor Cumming's contrivance, and the oil of Professor Clarke, would, I conceive, ensure perfect safety.

N.B. I have constructed a gaseous blow-pipe on the preceding principle. It is a circular vessel of cast-iron, with a flat top and bottom, eight inches in diameter, and four inches deep inside measure, and half an inch thick. At the bottom there is a circular hole five inches in diameter. Over this hole is placed a piece of thin lead, secured with an iron flange or ring, oiled leather, and screws. The reservoir stands upon four feet. Thus complete, it is placed over a hole in the table, &c. as before mentioned. Fitting up the apparatus myself, and requiring a valve to open into the reservoir, I made one, I think, on an improved principle. It is of brass, in the form of a cone, accurately ground into the lower extremity of the condenser, and secured with a piece of India rubber stretched through an eye on the base of the cone, and fastened with waxed thread, the elasticity of the India rubber keeping the valve perfectly close. By this means a valve might be put to the end of any small tube without increasing its diameter. I have tried this apparatus with condensed atmospheric air, and find the valve answer my utmost expectation.

I remain, dear Sir, yours, &c.

J. T. Beale.

XVII. *Another.*

(To Dr. Thomson.)

SIR, *Feb.* 8, 1817.

Observing in your number for this month a proposal for adapting to Newman's blow-pipe separate reservoirs for the gases, I think it right to acquaint you that the thing proposed has been already done by entwining jet pipes of two distinct blow-pipes of Newman's construction, and adapting a small cap with a capillary aperture to receive the separate gases, and re-issue them mixed. This instrument, which was ordered in November last, and completed some weeks ago, was constructed with a view to particular experiments, which are in progress; and the little that is peculiar in it bears reference to them, and need not now be explained. The principle is sufficiently simple. It may be enough at present to observe, that its performance is not equal to that of Newman's blow-pipe with a single reservoir; though the heat which it does produce is intense.

I am, Sir, your most obedient servant,

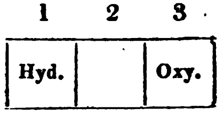
H. C.

XVIII. *Another.*

(To Dr. Thomson.)

DEAR SIR,

To the ingenious improvement proposed to Newman's blow-pipe in your last, I shall beg leave to suggest an additional one, which is as follows:—Instead of having two reservoirs, let there be but one, made in the annexed manner, with three partitions. 1 is the space designed for the hydrogen gas, and 3 that for the oxygen; 2 may be either empty, or filled with water.



B. P. certainly, Sir, deserves great praise for his proposition. It were well that every young chemist would exercise his ingenuity on this subject.

Feb. 7, 1817. R. S.

XIX. *R. M. A.'s Reply to the Queries of Civis.*

"The gentleman who signed himself R. M. A." feels no hesitation in giving distinct answers to the queries of Civis in the *Annals of Philosophy* for February, 1817.

To the first query, R. M. A. replies, that in the year 1814 he received from Col. Mudge himself the declaration, that the pendulum experiments at the different stations had long been a part of his proposed operations. R. M. A. does not now affirm, *nor did he in his communication of Oct.* 4, 1816, that the pendulum experiments, "as connected with the subject of weights and measures," were in Col. Mudge's contemplation. He apprehends that the Colonel's intentions were to make such experiments, with reference *to the determination of the figure of the earth:* but this is a point on which R. M. A. is not inclined to speak positively, however decisively he can aver as to the Colonel's *declaration.*

In answer to the second query, R. M. A. has to remark, that he *saw* at Woolwich, *more than a year and a half ago,* the astronomical clock which he mentioned in his former communication; and was then told for what purpose it was principally intended. As to "the operations which are *now* pursuing on that important subject," R. M. A. does not pretend to be in possession of the secret with respect to them, and he is unwilling to occupy the space of the *Annals of Philosophy* in the detail of mere rumours. If R. M. A., however, has been rightly informed, there are *no* "operations *now* pursuing" in consequence of the Resolution of the House of Commons to which Civis refers. Lord Stanhope's measures in the House of Lords rendered that resolution nugatory. The subject of pendulums for a while exercised the ingenuity of some of the mathematical Fellows of the Royal Society, and of Mr. Troughton, the deservedly celebrated astronomical instrument maker. But unless R. M. A. has been misinformed, every thing of this kind is now suspended; and it is doubtful whether it will be resumed, except by MM. Biot and Arago, in conjunction with Col. Mudge. Should these three philosophers employ, as is conjectured, a part of the present year in such operations, there can be no question, from the nature of their previous labours, that what they may thus accomplish will have reference *principally* (though, of course, not altogether) to the figure of the earth.

Feb. 1, 1817.

XX. *Lectures.*

Mr. Clarke will commence his next Course of Lectures on Midwifery, and the Diseases of Women and Children, on Thursday, March 20. The lectures are read every morning, from a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals, at the lecture room, No. 10, Saville-row, Burlington Gardens.

XXI. *System of Chemistry.*

Dr. Thomson has just commenced the printing of a new edition of his "System of Chemistry." The work will be entirely remodelled; and, it is expected, will be comprised in four octavo volumes.

ARTICLE XII.

METEOROLOGICAL TABLE.

1817.	Wind.	Barometer.			Thermometer.			Hygr. at 9 a. m.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
1st Mo.										
Jan. 10	Var.	30·58	30·40	30·490	30	20	25·0	82		☾
11	E	30·40	30·11	30·255	33	25	29·0	95		
12	S W	30·11	29·73	29·970	37	32	34·5			
13	S	29·73	29·47	29·600	38	32	35·0		5	
14	Var.	29·47	28·90	29·185	39	27	33·0		—	
15	Var.	29·31	28·75	29·030	33	19	26·0		—	
16	S W	29·31	28·83	29·070	39	24	31·5		·98	
17		29·00	28·83	28·915	42	37	39·5		7	●
18	S	29·03	28·99	29·005	45	37	41·0			
19	S	28·99	28·79	28·89	45	41	43·0	58	7	
20	S W	29·42	28·79	29·105	45	31	38·0	66	—	
21	S W	29·81	29·71	29·760	44	31	37·5	77	—	
22	S W	29·80	29·79	29·795	48	44	46·0	80	9	
23	S W	30·12	29·80	29·960	52	45	48·5	78	—	
24	S W	30·25	30·12	30·185	50	45	47·5	87	—	
25	S W	30·25	30·16	30·205	52	45	48·5	78	—	☽
26	S W	30·26	30·10	30·180	46	39	42·5	76		
27	Var.	30·38	30·31	30·345	50	39	44·5	92	6	
28	E	30·25	30·22	30·235	43	40	41·5	70	—	
29	Var.	30·29	30·24	30·265	47	37	42·0	75	—	
30	N	30·37	30·20	30·285	51	40	45·5	96		
31	N W	30·46	30·33	30·395	52	32	42·0	80		
2d Mo.										
Feb. 1	N W	30·44	30·41	30·425	49	34	41·5	75		
2	W	30·41	30·27	30·340	45	37	41·0	92		○
3	S W	30·27	29·95	30·110	41	38	39·5	70		
4	S W	29·69	29·50	29·595	43	35	39·0	57	2	
5	N W	29·90	29·69	29·795	50	38	45·0	73	4	
6	W	30·09	29·90	29·995	54	40	47·0	62		
7	W	30·21	30·09	30·150	51	42	46·5	63		
		30·58	28·79	29·846	54	19	40·03	76	1·38	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

First Month.—10. Fair: hoar frost: misty. 11. Much rime: very red *Cirrostrati* at sun-rise: in the course of the day the rime mostly came off the trees, with a S.W. wind. 12. Grey lofty sky: *Cirrocumulus*, p.m. 13. Misty: some rain after dark. 14. Clear, a.m. with *Cirrostratus* to S.: from whence afterwards came on cloudiness. 15. A considerable fall of snow from S.E., followed by sleet: snow at intervals, with a moderate breeze: clear frost at night. 16. Misty, gloomy, a.m. the wind very light, S.: then a steady breeze, S.W., and decided thaw, with much sleet and rain: the product of the rain-gauge is that of the gauge at the laboratory, my own having been accidentally overfilled. 17. The wind, for the first time in this period, blew a moderate gale in the night. 18. Fair day: somewhat windy night. 19. Fair: the wind E., with a lofty overcast sky, and much *scud:* at noon an electric-looking compound state of the clouds: after dark, rain from the southward, and a hard gale by morning. 20. Fine day: rain after dark: windy night. 21. Very fine day: a stiff breeze, with summer-like clouds in a blue sky: *Cirrostratus* at sun-set, and a lunar corona: windy night, and a dash of rain towards morning. 22. Drizzling at intervals: a gale at night. 23. Windy: a little rain, p.m.: at night a moderate gale. 24. *Cirrocumuli*, a.m. well formed from plumose *Cirri:* afterwards a pretty sudden obscuration, and some dripping. 25. Overcast: misty: a very little rain. 26. Ten minutes' sun about noon: the blackbird and robin sing much. 27. Misty and cloudy, as heretofore: at night the wind E., with moonlight and flying clouds. 28, a.m. Small rain: gloomy. 29. *Cumulostratus:* some sun at mid-day: at night wind N., with a veil of *Cirrostratus.* 31. Misty morning, followed by a very fine day, with *Cirrus* and *Cirrocumulus:* the hygrometer recedes to 52°.

Second Month.—1. Hoar frost: a fine day, with a gradation of clouds from *Cirrus* to *Cumulostratus*, ending in an overcast sky. 2. Grey sky. 3. Misty: cloudy. 4. *Cumulus* and sunshine: at evening, thick to the S.W.: the wind rose to a moderate gale, with a shower. 5, a.m. High wind, and clouds: dripping at night. 6. *Cirrostratus:* windy. 7. A fine sky of *Cirrocumulus:* windy, especially at night: the surface is considerably dried of late, and the roads tend to be dusty.

RESULTS.

Winds, with little exception, westerly: from the new moon to the first quarter, a S.W. wind, which was uniformly moderate by day, and increased in force in the night.

Barometer:	Greatest height	30·58 inches.
	Least	28·79
	Mean of the period	29·846
Thermometer:	Greatest height	54°
	Least	19
	Mean of the period	40·03
Mean of the Hygrometer		76°
Rain		1·38 inch.

The hygrometer, having undergone some repair, was exposed (after adjustment) for 24 hours before the observation of the 19th, which is probably, therefore, accurate. It appears that on this day there was a tremendous gale on the coasts of Devon and Cornwall, which did much damage, particularly at Plymouth.

Tottenham, *Second Month*, 15, 1817. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

APRIL, 1817.

Article I.

Biographical Account of the Right Reverend Richard Watson, D.D. F.R.S. Lord Bishop of Llandaff. By Thomas Thomson, M.D. F.R.S.*

THE subject of this memoir was born in August, 1737, at Heversham, in Westmorland, five miles from Kendal; in which town his father, a clergyman, was Master of the Free Grammar School. His father superintended the whole of his early education till the year 1754, when he went to Trinity College, Cambridge. Here he distinguished himself by close application to study, residing constantly till made a scholar in May, 1757. He became engaged with private pupils in November following; and took the degree of B. A. in January, 1759. On this occasion he held a distinguished place, being Second Wrangler. He was elected Fellow of Trinity College in October, 1760; and was appointed assistant tutor to Mr. Backhouse in November of the same year. He took the degree of M.A. in 1762; and was made Moderator for the first time in October following.

In the year 1764 he became a candidate for the Professorship of Chemistry. I have been told that the late Dr. Paley, who afterwards distinguished himself so much by his writings in the departments of moral philosophy and theology, was a candidate for the same chair. Neither of these eminent men had paid any previous attention to the study of chemistry. Dr. Paley boasted at the time that he was better acquainted with it than Dr. Watson; for he could perform one chemical process at least, since he knew how to make *red ink*; while his antagonist, he believed, did not know so much. Dr. Watson, however, carried the election; and began the study of practical chemistry with so much assiduity, that he very materially injured his health. I have been frequently amused with the history of his first chemical campaign. He could not succeed in his earliest attempts at experimenting. His retorts broke, his liquids were spilled, his clothes were spoiled. But by perseverance he at last got the better of his awkwardness, and acquired the art of experimenting with ease and elegance.

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* My principal object is to lay before my readers the obligations which Chemistry lies under to this eminent philosopher. My biographical facts are derived from a memoir in the Gentleman's Magazine, 1816, September, p. 274.

In 1767 he became one of the head tutors of Trinity College; and in October, 1771, he was appointed Regius Professor of Divinity, with the Rectory of Somersham, in Huntingdonshire, annexed. In 1774, he was presented to a prebend in the church of Ely; and in January, 1780, he succeeded Dr. Charles Plumtre in the archdeaconry of that diocese.

He had been tutor to the late Duke of Rutland when his Grace resided at Cambridge. In 1782 the Duke presented him to the valuable Rectory of Knaptoft, Leicestershire; and in the same year, through the recommendation of the same nobleman, he was advanced to the Bishoprick of Llandaff. In consequence of the smallness of the revenues of this Bishoprick, Dr. Watson was allowed to hold with it the Archdeaconry of Ely, his Rectory in Leicestershire, the Divinity Professorship, and the Rectory of Somersham. This Bishoprick was the last of his preferments. Though it is the poorest in the gift of the Crown, and though Dr. Watson was without doubt one of the greatest ornaments of the Bench of Bishops, and though his scientific and theological knowledge, the urbanity of his manners, and his uniform zeal for religion, would have made him fill with honour the first place in the English church, yet there were some circumstances which effectually prevented his promotion.

He had early associated himself with the political party known in Great Britain by the name of Whigs. During the King's illness in 1789, when the Regency Bill was before Parliament, he took an active part in advocating the right of the Prince of Wales to be appointed Regent, without limitation. During the American war he had been equally hostile to the ministerial party at that time in power, and argued the cause of the Americans with zeal and ability. At the commencement of the war with revolutionary France he was equally an enemy to hostilities; though long before the termination of that memorable struggle he had become sensible of its necessity, and urged its continuance and vigorous pursuit with much earnestness and eloquence. Thus his political sentiments during almost the whole of his life were at variance with those in whom lay the virtual distribution of promotions in the church. It is not surprising, therefore, that he was overlooked: though it would have redounded highly to the credit of Mr. Pitt, and his companions in the administration, if they had disregarded all political differences, and contributed to the promotion of one of the greatest ornaments of the church of England; but such want of magnanimity is rather an object of regret than of surprise.

Dr. Watson distinguished himself as a theological, political, and scientific writer. Of his theological writings the most important perhaps are his Apology for Christianity, and his Apology for the Bible. The first was published in 1776, as an answer to Gibbon's celebrated 15th chapter on the Causes of the Growth of Christianity. Of all the answers to Gibbon, this displayed the greatest urbanity and politeness, without any deficiency either of argument or spirit. The second was published in 1796, in answer to Thomas Paine's Age of Reason, which at that time was disseminated with particular industry through the British empire; and, being written in that peculiar style of which the author was such a consummate master, was particularly calculated to make an impression upon the common people. Indeed, the impression which it did make was wonderful, and almost instantaneous. The Apology for the Bible was written to counteract the effects of this insidious book in the only quarter where they could be dangerous; for the ignorance of Paine, and the absurdity of his arguments, were so obvious, that the book could make an impression only on those who were altogether ignorant of the subject, and had never thought of the evidences upon which their religion rested. Dr. Watson's answer, accordingly, was adapted to the understandings of the common people, and was a masterpiece, whether we consider the skill with which he overturned the insidious arguments of his antagonist, or the ability with which he counteracted the baneful effects of the principles which it was the object of the revolutionists to inculcate: for the avowed intention of these wild enthusiasts was to destroy the morality and religion of the common people altogether, and thus render them capable of going every length that might suit the objects of the demagogues of the day.

In 1785 Dr. Watson published a Collection of Theological Tracts selected from various Authors for the Use of the younger Students in the University, in six octavo volumes. This publication deserves high praise for the judgment with which the tracts were selected. They form a kind of theological library, which cannot but be very valuable to the theological student.

It will not be expected that I should notice here the different sermons which Dr. Watson published during the course of his long life. Indeed, I do not consider myself as at all qualified for such a task, having never seen several of them at all, and only glanced over the rest in a cursory manner.

The political publications of Bishop Watson are still less proper for discussion in a work like the present, which professes to be totally devoted to physical science. I may notice merely a few of his most prominent opinions. In 1783, immediately after his promotion to the Bishopric of Landaff, he published a letter to Archbishop Cornwallis on the church revenues, recommending a new disposition, by which the bishoprics should be rendered equal to each other in value, and the smaller livings be so far increased in income, by a proportionate deduction from the richer endowments, as to render them a decent competency.—From the commencement of the discussion respecting the slave-trade he was always a strenuous advocate for its abolition. He also exerted himself strongly in endeavours to repeal the Corporation and Test Acts.—His scheme for the abolition of the national debt, by every individual giving up a certain portion of his property, indicated less refined notions respecting political economy than might have been expected from a writer possessed of so much general knowledge upon so many subjects, and so conversant with the best writers of his time.

His chemical writings will claim a greater share of our attention. Indeed, it was to give an account of them that the present article was drawn up. In 1769 he was elected a Fellow of the Royal Society; and five papers by him were published in the subsequent volumes of the Philosophical Transactions. These papers were as follows:—

I. *Experiments and Observations on various Phenomena attending the Solution of the Salts.* (Phil. Trans. 1770, p. 325.)

This is a very elegant paper, and may serve as a model for the method of drawing up experimental investigations. It first made chemists acquainted with four sets of facts of considerable importance. 1. That, when salts are dissolved in water, they are not merely received into the pores of that liquid, but enter into chemical union with its particles. 2. The specific gravity of different salts. His mode of ascertaining that specific gravity was very simple, and yet susceptible of considerable accuracy. He took a globular glass vessel with a long narrow cylindrical neck. The neck was exactly graduated; so that the proportion which it bore to the capacity of the whole vessel was accurately known. The vessel being filled with distilled water up to a given mark in the neck, a certain weight of the salt whose specific gravity was to be determined was thrown into it. The water immediately rose in the neck of the vessel. From this rise it was easy to infer the specific gravity of the salt. The following table exhibits the results which he obtained in this way:—

Salts.	Specific Gravity.
Sulphate of soda	1·380
Crystals of kelp	1·414
Carbonate of ammonia	1·450
Sal-ammoniac	1·450

Salts.	Specific Gravity.
Sugar	1·487
White sugar candy	1·567
Acetate of potash	1·567
Glauber's salt from Lymington	1·657
Rochelle salt	1·757
Alum	1·757
Borax	1·757
Sulphate of iron	1·812
Sulphate of zinc	1·933
Nitre	1·933
Common salt	2·143
Sulphate of copper	2·230
Pearl ash	2·320
Sulphate of potash	2·636
Sulphate of iron calcined to whiteness	2·636
Carbonate of potash	2·761
Basket sea salt	3·052
Corrosive sublimate	4·142
Sulphate of mercury	6·444

The solution of all salts in water is greater than the mean of the specific gravity of the salt and of the water. Hence a condensation obviously takes place during the solution; and of course as the solution goes on, the water is constantly sinking in the neck of the matrass, though it never sinks so low as the mark at which it stood before the addition of the salt. We may infer from this fact that Dr. Watson's method for determining the specific gravity of salts answers worst for those that dissolve most rapidly in water.

3. The specific gravity of water at 41° saturated with different salts. The following table exhibits the facts which he ascertained:—

Substances in solution.	Sp. Gr. of solution.
Quick-lime	1·001
Crystals of tar	1·001
Arsenious acid	1·005
Borax	1·010
Corrosive sublimate	1·037
Alum	1·033
Sulphate of soda	1·052
Sulphate of potash	1·054
Common salt	1·198
Arsenite of potash	1·184
Glauber salt of Lymington	1·232
Sal-ammoniac	1·072
Carbonate of ammonia	1·077
Crystals of kelp	1·087
Nitre	1·095

Substances in solution.	Sp. Gr. of solution.
Rochelle salt	1·114
Sulphate of copper	1·150
Sulphate of iron	1·157
Sal-gemme	1·170
Sulphate of magnesia	1·218
Sulphate of zinc	1·368
Pearl ash	1·534

4. The specific gravities of water holding in solution $\frac{1}{12}$th of its weight of eight different salts. These are as follows. Temp. 40:—

Sea salt	1·059
Sulphate of soda	1·052
Nitre	1·050
Sulphate of zinc	1·045
Sulphate of iron	1·043
Lymington Glauber salt	1·039
Sulphate of soda	1·029
Sal-ammoniac	1·026

This paper contains, likewise, a table of the specific gravity of water impregnated with 37 different proportions of the common salt of commerce, varying from $\frac{1}{3}$ of the weight of the water to $\frac{1}{1084}$. This table is too well known to require insertion here.

II. *Remarks on the Effects of the Cold in February*, 1771. (Phil. Trans. 1771, p. 213.)

On Feb. 12, 1771, about an hour after sun-set, the thermometer at Cambridge stood at 6°. This cold seems to have come on rapidly, for the Cam was not frozen. Dr. Watson relates the state in which he found several saturated solutions of salts in his laboratory. But as he neglected to specify the temperature of these solutions when examined, the information conveyed in this paper is much less valuable than it otherwise would have been. Solutions of

Alum,
Cream of tartar,
Arsenious acid,
Corrosive sublimate,
Borax,
Nitre,

were entirely frozen. Those of

Sulphate of iron,
Sulphate of copper,
Rochelle salt,
Sulphate of soda,
Sulphate of zinc,

were nearly frozen, except the last, which contained only a few glacial spiculæ. Solutions of

Common salt,
Sal-ammoniac,
Carbonate of ammonia,
Carbonate of potash,
Sulphate of magnesia,
Lymington Glauber's salt,

were entirely fluid. A temperature of 6° being too high for the congelation of water saturated with salt, it is obvious that this solution could not have congealed. The subject of the congelation of solutions of different saline solutions was afterwards taken up, and very satisfactorily resolved by Sir Charles Blagden in a paper published in the Philosophical Transactions for 1788, to which I refer the reader. If we compare the freezing point at which the saturated saline solutions tried by Sir Charles Blagden congeal, with the state of Dr. Watson's solutions, we may conclude that their temperature was about 25½°; for the freezing point of a saturated solution of nitre is 26°, and that of a saturated solution of sulphate of magnesia 25½°.

III. *Account of an Experiment made with a Thermometer whose Bulb was painted black, and exposed to the direct Rays of the Sun.* (Phil. Trans. 1773.)

In the beginning of July, 1772, he exposed a thermometer to the direct rays of the sun. It rose to 108°. On blackening the bulb by means of China ink, the thermometer rose to 118°. This experiment was carried much further by De Saussure, and the late Professor Robison, of Edinburgh. Saussure, by surrounding the bulb with charred cork, and exposing it to the sun's rays, made it rise to 221° at Geneva. Professor Robison, at Edinburgh, raised a thermometer by similar means to 237°.

IV. *Chemical Experiments and Observations on Lead Ore.* (Phil. Trans. 1778.

In this paper he gives the specific gravity of different varieties of galena. He shows that it may be sublimed in close vessels; but that the highest temperature to which it can be raised is insufficient to decompose it. But when air is admitted to galena at a white heat, sulphur escapes from it, and the ore is partially reduced to the metallic state. He shows that in galena the lead is in the metallic state. He attempted the analysis of galena by the action of diluted nitric acid. By this means he dissolved the lead, and obtained a quantity of sulphur which amounted in his trials to between ⅛ and ⅑ of the weight of the ore. The real proportion of sulphur in galena is $\frac{1}{7\cdot5}$ of the whole. He found that Chinese lead, when melted, would not form the colours on its surface which are known to distinguish pure lead. He made a great many experiments to discover the cause of this difference, and at last ascertained that when a little tin is added to melted lead, it deprives it of the power of exhibiting colours on its surface. I found some years ago that the lead which lines tea boxes contains about four per cent of tin. Zinc produces the same effect upon lead; but not bismuth or silver. The order in which the colours appear on melted lead is as follows: *yellow, purple, blue—yellow, purple, green—pink, green—pink, green.*

V. *Observations on the Sulphur Wells of Harrogate, made in July and August,* 1785. (Phil. Trans. 1786.)

In this paper Dr. Watson describes the situation of the four Harrogate wells, their specific gravity, and the experiments which he made to determine their saline and gaseous ingredients. In the present state of our chemical knowledge of mineral waters, it would be useless to state the result of these experiments. It may be sufficient to say that the author's opinions were accurate as far as they went; that he knew of the existence of sulphureted hydrogen, and that it could be obtained by the solution of certain metallic sulphurets in muriatic acid.

In the year 1781 Dr. Watson published his Chemical Essays, in three 8vo. volumes. This work was highly popular at the time of its appearance, and contributed very materially to produce that taste for chemical science which at present so generally pervades Great Britain. These essays are beyond dispute the most elegant work on chemistry which has appeared, either in this country or the continent of Europe. Though the science has undergone two complete revolutions since 1781, and though the progress which every branch of it has made since that period is quite enormous, yet these essays have not yet lost their interest, and will always retain a considerable portion of value, because they contain a great collection of historical facts not to be found any where else. The essays on gunpowder, on the saltness and temperature of the sea, on the quantity of water evaporated from the surface of the earth in hot weather, on the smelting of lead ore, on silver extracted from lead, and on red and white lead, may be still perused with interest by the chemists of the present day, and deserve to be studied as models of elegant memoirs on scientific subjects. It would far exceed the limits to which I must confine myself here, were I to attempt an analysis of the contents of these volumes. But I consider the task as quite unnecessary, as they must be familiar to every chemist of taste in the British empire.

In the year 1786 he published a fourth volume of Chemical Essays, which I consider as much more valuable than the preceding ones. It contains a great stock of very valuable information respecting the progress of various chemical manufactures in Great Britain, chiefly concerning the smelting of some of the metals, and forming some of the most important alloys. His account of blende, zinc, brass, gun-metal, tinning copper and iron, and gilding in *or moulu*, are highly interesting. To this volume he soon after added a fifth, containing his essays which had been published in the Philosophical Transactions, and one or two others of inferior consequence.

It is very much to be regretted that Dr. Watson did not continue to prosecute the science of chemistry, which he had begun to cultivate with so much ardour, and which his erudition and sagacity fitted him so well to have improved, and his elegant style made him so well qualified to have explained.

Article II.

An Essay on the Oopas, or Poison Tree of Java, addressed to the Honourable Thomas Stamford Raffles, Lieutenant Governor. By Thomas Horsfield, M.D. (Communicated to the Society by the President.)

(*Concluded from* p. 214.)

Exper. 15.—Having, by the assistance of the commandant of Banjoowangee, obtained from the Island of Bali an arrow, supposed to be armed with the oopas from Borneo, I wounded a dog in the muscles of the thigh. On the 10th minute he became restless, attempted to extricate himself, and barked. In 14 minutes he extended his tongue, had an increased flow of saliva, shewed a disposition to vomit. In 15 minutes he was very much agitated, jumping, barking, and making violent efforts to escape; the attempts to vomit became more repeated. In 25 minutes he appeared exhausted and extended his limbs. In 30 minutes the muscles of the abdomen were contracted. In 32 minutes he vomited. In 37 minutes he vomited an excremental matter. In 40 minutes he breathed heavily and laboriously; the muscles acted violently. In 45 minutes lying exhausted and breathing hastily. In 50 minutes he started suddenly and barked. In 55 minutes he cried out violently, and having discharged his excrement, after a few interrupted respirations, he died. On dissection the same appearances were observed as after the above related experiments.

Exper. 16.—I obtained a small quantity of the oopas of the Island of Borneo, which having moistened, and rendered somewhat fluid with cold water, I applied to a dart, and wounded a dog in the usual manner. The first three minutes he appeared little affected by the wound. On the fifth minute he shewed symptoms of drowsiness, which gradually increased. In six minutes he staggered and reeled round. In 10 minutes the drowsiness returned, after which he reeled round again. He now had an increased flow of saliva, and his breathing became quicker. In 12 minutes he reeled round again with more violence, and trembled. On the 14th minute he fell down with violent tremors and extended his extremities convulsed: after a short calm, the symptoms recurred with greater violence on the 15th minute, when after violent tremors, convulsions, and screaming, he died.

A creeping undulatory motion was observed in the skin after death over the surface of the whole body in this and several other instances.

Exper. 17.—The following experiment was made at Soorakarta (in the course of the month of March, 1812) with the poison of the antshar, which I collected at Banjoowangee, in July, 1806. A dog of middling size was wounded in the usual manner in the muscles of the thigh with a dart that had been dipped into the poison about 24 hours before, and during the interval had been exposed to the open air of a chamber. During the first 20 minutes after the puncture he remained quiet, and shewed few symptoms of uneasiness, except a kind of heaviness and fatigue: on the 20th minute his abdominal muscles were somewhat contracted, and he breathed heavier. In 25 minutes he had an increased flow of saliva and licked his jaws. In 27 minutes he started, screamed violently, fell down convulsed, and discharged the contents of his rectum. On the 28th minute the convulsions returned violently, and continued without interruption till the 30th minute, when he died.

The dissection agrees with those previously made. The stomach was distended: it contained the food previously taken, the poison having acted with uncommon violence it was not ejected as usual. In the thorax the large vessels were very much distended with blood, exhibiting the appearances above described.

The vessels of the lungs were distended and the lungs were florid.

On removing the cranium the brain and dura mater were found nearly natural, the former pale, and perhaps more watery than usual.

II. *Experiments with the Tshettik.*

Exper. 18.—A dog of middling size was wounded in the muscles of the thigh with a dart covered with the fresh prepared poison of tshettik. In two minutes he shewed symptoms of uneasiness; he appeared faint and lay down. In three minutes and a half he was seized with convulsive twitchings of the extremities, was very restless, and his breathing became quick: these symptoms gradually increasing to the sixth minute, while he continued as exhausted in a lying posture. He now raised himself, extended his head as if attempting to leap, but fell down, was seized with violent convulsions, attended by quick and interrupted breathing to the ninth minute, when he died.

Exper. 19.—A small dog was wounded in the usual manner in the muscles of the thigh with the poison of tshettik. He immediately placed himself in a drooping posture, his fore-legs bent as in kneeling, and thus he continued to the fifth minute; he was now seized with trembling, which continued about half a minute, when he suddenly started, extended his head and neck, stretched out his extremities, and falling on his side, was violently convulsed. His legs continued stiff, extended and trembling. These symptoms continued with great force until the eighth minute, when they gradually diminished; his respiration became interrupted; he had occasional twitchings to the 11th minute, when he died quietly.

On dissection the contents of the abdomen were found perfectly natural; the stomach was distended with food newly taken in. In the thorax the heart and lungs appeared natural; the aörta was almost empty, and on being punctured a small quantity of blood ran out of a dark colour: the ascending and descending venæ cavæ were distended with dark blood, which being let out, soon coagulated in the cavity of the thorax. The brain was most affected; the vessels were distended and inflamed, the sinuses were filled with dark coloured blood.

Exper. 20. A fowl nearly full grown was pierced through the muscles of the thigh with an arrow armed with tshettik. After the first impression was over, it seemed insensible to the wound about one minute, walking round and picking up grains as usual; near the second minute it became giddy, and unable to stand, placed itself into a half-sitting posture. On the third minute it began to breathe hastily. In five minutes it trembled, and discharged the contents of its bowels. It now made an attempt to rise, and extended its head and neck; but being unable to support itself, reeled round, fell down, had violent convulsions, with quick interrupted breathing, which continued to the ninth minute, when it died.

Exper. 21.—A fowl was wounded with a poisoned dart in the back near the left wing, the puncture extending towards the cavity of the thorax. In less than one minute it shewed some uneasiness and could with difficulty support itself. In one minute and a half it had a fluid discharge from the bowels, after which it suddenly started, extended its head and legs, and trembled violently, fluttering with the wings. On the third minute it made a sudden effort to run, and extended its neck, but fell down head foremost, and was violently convulsed, fluttering with the wings; the respiration was extremely laborious, and soon became interrupted; the convulsions continued to the fourth minute, when it died.

Exper. 22.—A fowl was wounded in the usual manner with an arrow covered with the oopas of tshettik, which had not been mixed with the spices employed in the preparation. On the 40th second it felt the operation pricking its breast violently, as if it perceived an itching. In one minute it reeled round. In one minute and a half it extended its neck, fell down forwards, fluttered, and was seized with convulsions, which continued to the third minute, when it died.

Exper. 23.—The following experiment was made in August, 1808, two years after the preparation of the poison. A fowl was wounded in the usual manner with a poisoned dart. It died with the above related symptoms two minutes after the puncture.

Exper. 24.—I infused a small portion of the bark of the tshettik in alcohol: having macerated it a few days I exposed it to the open air for co-operation, and obtained a small quantity of an elegant brown shining resin. A dart was covered with a few grains of this, and a fowl wounded in the usual manner. The first three minutes after the puncture it remained quiet and appeared drooping. On the fourth minute it reeled backward, tottered, and its limbs were relaxed. On the sixth minute it appeared to be sleepy, but its drowsiness was frequently interrupted by twitchings and startings. In eight minutes it tottered, but soon became drowsy again. In 12 minutes it fell down convulsed and trembling, but soon became quiet, and its breathing was quick. On the 17th minute it had occasional twitchings in the extremities, and was unable to stand erect. On the 20th minute the drowsiness had considerably diminished; it rose, and supported itself, but tottered in attempting to walk. From the 30th minute it began to revive, all the effects gradually went off, and on the 60th minute it was apparently well.

Exper. 25.—The following experiment was made at Soorakarta in the month of March of the present year 1812, nearly six years after the collection of the oopas in Blambangan. A dog of middling size was wounded in the muscles of the thigh with a dart, which having been dipped into the oopas, was exposed half an hour to the open air, to give the poison time to become dry. During the first two minutes he stood quiet, and his appearance only exhibited the pain produced by the wound. On the third minute he was drowsy. In five minutes he began to tremble violently and to reel. On the seventh minute he fell down head foremost and was convulsed, his extremities being stiffly extended: unable to raise himself again, the convulsions continued with excessive violence till the ninth minute, when he died.

On dissetion his stomach was found natural and contained the food lately taken in: all the viscera of the abdomen were also natural. In the thorax the venæ cavæ were found completely filled, and the aörta partially filled with blood, the lungs still retained a florid colour. On removing the cranium and exposing the brain the whole surface of the dura mater was found inflamed, and the vessels were injected with blood; that part covering the right lobe in particular was in a state of the highest inflammation; it exhibited externally a livid bluish colour: on the internal surface (of the dura mater) the fluid had been forced out of the vessels by the violence of the action, and it was covered by a bloody lymph. The integuments of the cerebellum were also strongly affected. In the vessels of the surface of the brain itself some marks of inflammation were also perceived. On tracing the wound no evident marks of inflammation appeared, and the remains of the adhering poison were evident along its course.

Exper. 26.—(To shew the effects of the poison taken *internally.*) To a nearly full grown dog about half the quantity of poison generally adhering to a dart was given in a little boiled rice. During the first 10 minutes he remained quiet and appeared a little drowsy: on the 14th minute he could with difficulty support himself erect, and indicated symptoms of pain; he shewed some disposition to vomit, and extended his jaws. In 28 minutes he extended his hind legs spasmodic. In 31 minutes he had violent spasms over his whole frame. In 37 minutes he stood breathing hastily, his abdomen appeared uneasy. In 39 minutes he had spasmodic extensions of his extremities, which lasted half a minute, when he became quiet; but being faint, supported himself against a wall. In 46 minutes he started up convulsed. In 48 minutes he appeared oppressed in the head and drowsy. In 54 minutes he started up suddenly. In 60 minutes he appeared oppressed and drowsy. In 61 minutes he fell backwards in violent convulsions, his extremities strongly contracted by spasms, after which he became calm. On the 63d minute, being roused and attempting to walk, he fell backwards with violent spasms and convulsions. In 65 minutes, having raised himself with difficulty, he stood with his extremities far extended, and his muscles in a state of spasmodic contraction. In 67 minutes he fell down head-foremost, violently convulsed, his breathing became interrupted, and on the 69th minute he died.

Dissection.—On opening the abdomen several ounces of a clear serous fluid, mixed with streaks of newly coagulated blood, were found effused in the cavity; the vessels of the external coats of the stomach of the intestines and mesentery were in the highest possible degree inflamed, and distended beyond their natural size, having evidently been acted on by the most violent force; the stomach being opened was found empty, its internal coat was corrugated and covered with frothy mucus in which were found the remains of the poison, a dark yellow fluid with some grains of the rice with which it was conveyed. In the thorax the lungs were still florid, the venæ cavæ much distended, the aörta nearly empty; being punctured the blood flowed out of a dark hue.

On exposing to view the brain, the dura mater was nearly natural, only the larger vessels somewhat more distended than usual: the vessels of the brain itself indicated a slight degree of inflammation.

Remarks on the Experiments.—I have selected from a large number of experiments, those only which are particularly demonstrative of the effects of the antshar and of the tshettik when introduced into the circulation. The poison was always applied by a pointed dart or arrow made of bamboo. The extremity to which the poison adhered was completely spear-shaped, about an inch long, and a line and a half broad near the middle of its length.

When I contemplated an experiment, the dart was dipped into the fluid poison, which I preserve in closed vessels. It is necessary to give it some time to become dry and fixed upon the dart. I found by repeated trials the poison most active after having adhered 24 hours to the weapon; if applied in a fluid state if does not enter the wound in a sufficient quantity to produce its effects, but in the attempt to thrust it through the muscles, it separates itself from the dart, and adheres externally to the integuments.

The operation of the two different poisons on the animal system is essentially different.

The first 17 experiments were made with the antshar; the rapidity of its effect depends in a great degree on the size of the vessels wounded, and on the quantity of poison carried into the circulation.

In the first experiment it induced death in 26 minutes; in the second, which was made with the sap collected in Poogar, in 13 minutes. The poison from different parts of the island has been found nearly equal in activity.

In the ninth experiment, with the poison from Passooroowang, death followed in 29 minutes.

The common train of symptoms is a trembling and shivering of the extremities, restlessness, erection of the hair, discharges from the bowels, drooping and faintness, slight spasms and convulsions, hasty breathing, an increased flow of saliva, spasmodic contractions of the pectoral and abdominal muscles, retching, vomiting, excremental vomiting, frothy vomiting, great agony, laborious breathing, violent and repeated convulsions, death.

The effects are nearly the same on quadrupeds, in whatever part of the body the wound is made. It sometimes acts with so much force, that not all the symptoms enumerated are observed; in these cases, after the premonitory symptoms (tremors, twitchings, faintness, and an increased flow of saliva,) the convulsions come on suddenly, and are quickly followed by death.—See the 17th experiment.

The oopas appears to affect different quadrupeds with nearly equal force, proportionate in some degree to their size and disposition. To dogs it proved mortal in most experiments within an hour; a mouse died in 10 minutes, see Exper. eighth; a monkey in seven minutes, see Exper. 11th; a cat in 15 minutes, see Exper. 12th.

A buffalo, one of the largest quadrupeds of the island, died in two hours and 10 minutes, see Exper. 13th. I do not think the quantity of poison introduced in this experiment was proportioned to that which was thrown into the system in the experiments on smaller animals; the dart fell from the wound before a sufficient quantity had been taken into the circulation to produce a rapid effect. If an animal is pierced by an iron spear to which the poison has been applied, it feels comparatively but little of the effects, because the weapon is again retracted, and the poison does not remain in contact with the wound long enough to be taken into the circulation. Mr. Leschenaut de la Tour stabbed a buffalo a number of times successively with a common spear or pike of the Javanese, largely covered with the poison of the tshettik, without very sensibly affecting the animal. A dart or arrow prepared of bamboo is a more fit instrument to introduce the oopas; having once pierced the skin, it easily adheres to the parts it comes in contact with, on account of its inconsiderable weight.

The natives of Macasser, Borneo, and the Eastern Islands, when they employ this poison, make use of an arrow of bamboo, to the end of which they attach a shark's tooth, which they throw from a blow-pipe or *sompit.*

The 15th and 16th experiments are *comparative*, they were made with the oopas from Bali and Borneo; by contrasting them with the first, second, ninth, and 17th experiments, it sufficiently appears how far the oopas of the different islands agrees in activity. It is probable that the oopas from Borneo, when fresh, may act more forcibly than that of Java.

If the simple or unprepared sap is mixed with the extract of tobacco or stramonium, (instead of the spices mentioned in the account of the preparation), it is rendered equally, perhaps more active.—See the third and fourth experiments.

Even the pure juice unmixed and unprepared, appears to act with a force equal to that which has undergone the preparative process, according to the manner of the Javanese at Blambangan. See the fifth experiment made with the fresh juice of Banjoowangee, and the 10th experiment with the fresh juice collected at Goorrong, near Passooroowang.

Birds are very differently affected by this poison; fowls have a peculiar capacity to resist its effects. In the 44th experiment a fowl died 24 hours after the wound, others have recovered after being partially affected.

The sixth and seventh experiments shew the effects of the unprepared juice on two birds of the genus ardea.

The 18th and the succeeding experiments were made with the poison prepared from the tshettik. Its operation is far more violent and rapid than that of the antshar, and it affects the animal system in a different manner; while the antshar operates chiefly on the stomach and alimentary canal, the respiration and circulation, the tshettik is determined to the brain and nervous system.

A relative comparison of the appearances on dissection demonstrates in a striking manner the peculiar operation of each.

The 18th, 19th, and 25th experiments give a general view of the effects of the tshettik on quadrupeds.

After the previous symptoms of faintness, drowsiness, and slight convulsions, it acts by a sudden impulse, which, like a violent apoplexy, prostrates at once the whole nervous system.

In the 18th and 19th experiments this sudden effect took place on the sixth minute after the wound, and in the 25th experiment on the seventh minute, the animals suddenly started, fell down head-foremost, and continued in convulsions till death ensued.

This poison affects fowls in a much more violent manner than that of the antshar, as appears from the 20th and 21st experiments; they are first affected by a heat and itching of the breast and wings, which they show by violently picking these parts; this is followed by a loose discharge from the bowels, when they are seized with tremors and fluttering of the wings, which having continued a short time, they fall down head-foremost, and continue convulsed till death. I have related such experiments as show the gradual operation of the poison; in some instances (especially in young fowls) it acts with far greater rapidity; death has frequently occurred within the space of a minute after the puncture with a poisoned dart.

It appears from the 22d experiment, that the simple unmixed decoction of the bark of the root of the tshettik is nearly as active as the poison prepared according to the process above related.

The 24th experiment shows plainly, that the resinous portion of the bark is by no means so active as the particles soluble in water; a fowl wounded by a dart covered with the pure resin, recovered after being very partially affected; it has also been remarked above, that in the preparation of the dried juice of the antshar, the resinous parts are thrown away. The strength of the poison remains unimpaired, if carefully preserved a number of years, as is evident from the experiments made at different periods of its age.

Taken into the stomach of quadrupeds, the tshettik likewise acts as a most violent poison, but it requires about twice the period to produce the same effect which a wound produces.

In the 26th experiment its operation internally is detailed, and the appearances after death are described in the account of the dissection.

But the stomach of fowls can resist its operation; having mixed about double the quantity generally adhering to a dart with the food of a fowl, it consumed it without showing any marks of indisposition.

The poison of the antshar does by no means act so violently on quadrupeds as that of the tshettik. I have given it to a dog; it produced at first nearly the same symptoms as a puncture; oppression of the head, twitchings, faintness, laborious respiration, violent contraction of the pectoral and abdominal muscles, an increased flow of saliva, vomiting, great restlessness and agony, &c., which continued nearly two hours; but after the complete evacuation of the stomach by vomiting, the animal gradually recovered.

Rumphius goes so far as to assert that a small quantity may be taken internally as a medicine. In speaking of the qualities of the arbor toxicaria, he says the crude and unmixed ipo is an antidote to the bite or sting of venomous fishes and insects: also, that a person affected by an eruption of the skin or vacuations, may take a small pill of the oopas, which will attract all impurities from the intestines and carry them off.

The appearances observed on dissection explain in a great degree the relative operation of the poisons. In animals killed by the antshar, the large vessels in the thorax, the aörta, and venæ cavæ, were in every instance found in an excessive degree of distention:

the viscera in the vicinity of the source of circulation, especially the lungs, were uniformly filled in a preternatural degree with blood, which in this viscus and in the aörta still retained a florid colour and was completely oxygenated. On puncturing these vessels it bounded out with the elasticity and spring of life. The vessels of the liver, of the stomach, and intestines, and of the viscera of the abdomen in general, were also more than naturally distended, but not in the same degree as those of the breast. In the cavity of the abdomen a small quantity of serum was sometimes effused.

The stomach was always distended with air, and in those instances in which the action of the poison was gradual, and in which vomiting supervened in the course of the symptoms, its internal coat was covered with froth.

The brain indicated less of the action of the poison than the viscera of the thorax and abdomen. In some instances it was perfectly natural, in others marks of a small degree of inflammation were discovered.

An undulatory motion of the skin and of the divided muscles was very evident in some of the dissected animals.

The appearances observed in the animals destroyed by the tshettik were very different. In a number of dissections the viscera of the thorax and abdomen were found nearly in a natural state, and the large vessels of the thorax exhibited that condition in which they are usually found after death from other poisons.

But the brain and the dura mater shewed marks of a most violent and excessive affection. In some instances the inflammation and redness of the dura mater was so strong, that on first inspection I supposed it to be the consequence of a blow previously received, until I was taught by repeated examinations that this is a universal appearance after death from tshettik.

I am not at present at leisure, nor am I properly prepared, to investigate fully the operation of the two poisons described on the animal system, or to elucidate their effects by a comparison with other poisons. The series of experiments I have proposed to myself, and which are necessary for the purpose, is by no means finished, nor does my situation at present afford me those opportunities of scientific consultation, which such an investigation requires; it remains for a futute period also, to determine, relatively, the force of these poisons with that of the most venomous serpents; the tshettik exceeds, perhaps, in violence, any poison hitherto known. It shows its effects peculiarly, and almost exclusively, on the brain and nervous system.

The action of the antshar is directed chiefly to the vascular system. The volume of the blood is accumulated in a preternatural degree in the large vessels of the thorax.

The circulation appears to be abstracted from the extremities and thrown upon the viscera near its source. The lungs in particular are stimulated to excessive exertions. The balance of circulation is destroyed. The vital viscera are oppressed by an intolerable load, which produces the symptoms above described, while in the extremities a proportionate degree of torpor takes place, accompanied by tremors, shivering, and convulsions.

I have but little to add concerning the operation of the antshar on the *human* system, the only credible information on this subject is contained in the work of Rumphius, who had an opportunity of personally observing the effect of the poisoned darts or arrows, as they were used by the natives of Macassar in their attack on Amboina, about the year 1650.

They were also employed by the inhabitants of Celebes in their former wars with the Dutch. Speaking of their operation, he says the poison touching the warm blood, is instantly carried through the whole body, so that it may be felt in all the veins, and causes an excessive burning, and violent turning in the head, which is followed by fainting and death.

The poison (according to the same author) possesses different degrees of violence, according to its age and state of preservation.

The most powerful is called oopas radja, and its effects are considered as incurable; the other kinds are distributed among the soldiers on going to war. After having proved mortal to many of the Dutch soldiers in Amboina and Macassar, they finally discovered an almost infallible remedy in the root of the crinum Asiaticum (called by Rumphius radix toxicaria) which if timely applied, counteracted by its violent emetic effect the force of the oopas.

An intelligent Javanese at Banjoowangee informed me, that a number of years ago, an inhabitant of that district was wounded in a clandestine manner by an arrow thrown from a blow-pipe, in the fore arm near the articulation of the elbow. In about 15 minutes he became drowsy, after which he was seized with vomiting, became delirious, and in less than half an hour he died.

From the experiments above related on different quadrupeds, we may form an analogous estimate of its probable effects on man.

ARTICLE III.

Experiments on the Strength of different Kinds of Wood for ascertaining the Law of the Resistance, or how much more Weight Wood will sustain when the Breadth and Thickness are augmented. Also Observations on the Practice of staying Masts, and additional Remarks communicated by Mr. George Smart on the same Subjects. By Col. Beaufoy, F. R. S. With a Plate.

(To Dr. Thomson.)

MY DEAR SIR, *Bushey Heath, Dec. 2,* 1816.

I MADE the following experiments to ascertain the strength of different kinds of timber, and to determine how much the power of beams is increased or decreased by augmenting or diminishing their depth and breadth. Those already made did not satisfy my mind; but should these appear to you likely to interest any of your readers, I beg you to insert them in the *Annals*, though accuracy is the only merit they can boast.

The tables are divided into four columns: the first contains the weight employed to break the wood under trial, from 14 lb., 28 lb., avoirdupoise, and so on, to that which broke the wood: in the second column are the degrees and minutes, the timber bent by the application of the weights: in the third, the differences of the curvature of the pieces of wood: and the last column contains remarks. It is remarkable that in different parts of the same tree there is so great difference in the strength of the timber; for instance in the Dantzic oak, the strongest piece would sustain the mean weight of 136$\frac{1}{10}$ lb., another piece of the same tree 76$\frac{3}{10}$ lb., and another but 63$\frac{3}{10}$ lb.: in this last piece, however, there was a shake or imperfection: the heart of the oak sustained the mean weight of 84 lb. The differences of the curvature are tolerably regular till loaded with more than half the greatest weight the piece would carry; then the curvature, though irregular, becomes greater than in proportion to the weight added. From these trials, the pitch pine appears the strongest wood; next to that, the English oak with straight and even fibres, as the experiments on the six pieces prove; then the English oak irregular and cross grained; fourthly, the Riga fir; and, sixthly, the Dantzic oak. Thus the strength of pitch pine (by first experiment) is to the strength of

English oak ....as 148·44 to 145·5, which is as 101 is to 99.

(by second experiment)—

English oak.... as 148·44 to 128·66, which is as 15 is to 13
Riga fir ...... as 148·44 to 116·01, which is as 83 is to 65
Dantzic oak .. as 148·44 to 98·4, which is as 86 is to 57

and to the mean strength of

English oak....as 148·44 to 137·08, which is as 13 is to 12.

If the strength of the pitch pine be called 1000, the strength of the English oak by

First experiment will be	980
Second experiment	867
Mean of the two	923
Of the Riga fir	782
Of the Dantzic oak	663

Call the mean strength of the English oak 1000; the strength of Riga fir will be 846; but the weight of the Riga fir is to that of the English oak as 659 to 1000. Therefore the decrease of weight being in greater proportion than the increase of strength proves that in dry places it is better to use fir beams than oak, independently of the saving of expense.

The next set of experiments were made to find how much the strength of timber is increased by augmenting the depth and breadth, and what law this increase of dimensions follows. Several pieces of red fir were taken, each piece five feet long, and varying from 2, $2\frac{1}{4}$, $2\frac{1}{2}$, and 3 inches, in breadth and thickness. The mean strength of the first was 129 lb.; the second, 193 lb.; the third, 259 lb.; the fourth, 367 lb.; and the fifth, 440 lb.

To find the law of the increase of strength, try in what powers of thickness the several weights or resistances of the wood are, by comparing every two experiments; first 129 with 193, and then with 259, &c. and so on till every combination of two has been compared. This may be done by the following theorem:— $T^m : t^m :: W : w$. T and t representing the thickness of the timber, W and w the weights employed to break it, and m the exponent sought; $m = \frac{\text{Log. } W - \text{Log. } w}{\text{Log. } T - \text{Log. } t}$.

By applying this rule to all the numbers above, the several values of m, or exponent of the power, are as follows:—

129 & 193	3·4206						
	3·1236	193 & 259	2·7917				
	3·2833	367	3·2026	259 & 367	3·6569		
	3·0260	440	2·8645	440	2·9438	367 & 440	2·0849

The imperfections of the different pieces of wood cause these various values of m from 3·6569 to 2·0849. Add the 10 exponents together, and divide the sum by 10; the quotient 3·03979 may be considered as the mean value of m. The nearest calculated exponent to this is 3·0260, found from the numbers 129 and 440; which shows that the strength of timber increases in somewhat greater proportion than the cube of the thickness.

Assuming, therefore, either of these two numbers, 440 and 129, and their respective thickness, three and two inches, and computing the weights answering to either thickness, the unavoidable irregularities in the experiments will be corrected, and the strength brought into a regular series. Computing in this way, they come out as follows:—

T = 3 Log. 0·4771213
t = 2 Log. 0·3010300

0·1760913 Log. 1·2457347

3·0398 Log. 0·4828450

1·7285797 Nat. Numb. 0·53528

440 Log. 2·6434527

2·1081727 Nat. N. 128·28 lb.

which is the corrected strength answering to two inches. By proceeding in a similar manner with the other thicknesses, the corrected series will be as set down in the following table:—

Experimented strength ..	129	193	259	367	440
Regular series............	128·28	183·51	252·79	372·67	440

A piece of Dantzic oak two inches square, and projecting, like a beam, four feet from a wall, sustained the mean weight of $98\frac{4}{10}$ lb. hung on its extremity. The following rule will give how much the original piece would have borne, out of which the 25 pieces of two inches square were cut.

$T = 10$ Log. 1·0000000
$t = 2$ Log. 0·3010300

3·6989700 Log. 1·8444585

Mean Exp. 3·0398 Log. 0·4828450

0·3273035 Nat. N. 2·1247

W. 98·4 Log. 1·9929951

	lb.		T.	Cwt.	lb.
4·1176951	13·113	or	5	17	9

the calculated weight. But had the weight been calculated according to the cube of the thickness, it would have given 12300. The mean weight which the 25 pieces of Dantzic oak actually bore was 2459; then 12300 divided by 2459 leaves a quotient of 5·002; 5 is the square root of 25, which proves that in this instance the strength of a piece of timber when divided into many pieces decreases as the square root of the number into which it is divided, supposing each piece square and of the same size. The experiments also with the 25 pieces of pitch pine gave the quotient 4·9999, which is so near that it may be considered as 5. From these experiments are deduced the following practical conclusions. That in made and solid masts, or any other assemblage of pieces of wood, the pieces should be as few as possible. That in square beams the lateral strength is as the cubes of their breadth. That when the beam is not square, the strength is as the breadth multiplied into the square of the depth. Prior to the wood breaking, the fibres on the upper side are stretched, and those on the under side compressed. On the weights being taken off, the wood did not recover its horizontal position; and reversing the wood, it broke in the contrary direction with less weight.

These facts respecting the decrease of the strength of timber by the compression of the fibres on one side, and their consequent elongation on the opposite, prove how destructive to the service is the bad practice of violently staying forward the lower masts of large ships. By this method, masts are often crippled prior to the vessels going to sea. The mast is complained of, and rejected as a bad stick, replaced at the expense of several hundred pounds, and to the delay of the service. In the second set of experiments, the usual method of forming ship beams by splicing up and down is found to be the strongest, as the mean weight of the first and second $\frac{67{\cdot}7 + 67{\cdot}3}{2} = 67{\cdot}5$, is greater than that of the third and fourth, 66·55, or fifth and sixth, 58.

A few days past I received the following ingenious experiment and information from Mr. George Smart, carpenter and builder on the Surrey side of Westminster Bridge, and well known for his invention of hollow masts, yards, booms, bowsprits, oars, and sweeps; and for his valuable application of Mr. Bramah's press to compress wood, and prevent its shrinking when formed into staves for making gunpowder-barrels, canteens, &c. Neither should his peculiarly ingenious manner of forming saddle-trees be omitted. I beg to transmit you his own letter and drawing (Pl. LXV. Fig. 2):

" *Description of a Frame for making Experiments on the Strength of Timber.*

A, A, two planks of fir, one the top, and the other the bottom, of the frame, into which are framed the two upright ends, B, B, with double tenans. The bottom tenans are pinned, and the shoulders, as they are called by carpenters, housed about half an inch into the bottom plank. To give them more strength, the top plank, A, is morticed, to receive the top tenans and the wedges, C, C. There is a hole cut in each end, to admit the thick ends of the lath, L, which, being put into its place, is kept down by the blocks, D, D. The wedges, C, C, being driven tight, the lath, L, having the shoulder at each end, cannot slip; and is so strained that on its horizontal position it will carry nearly as great a weight as it would suspend in a vertical direction. It is now nearly 17 years since I first put the lath into the frame; and, to all appearance, it is as strong as when first put in. The late ingenious Dr. Anderson was on a visit at my house when I tried the first experiment. He told me that Belidor affirmed, that timber made fast at both ends would carry one-third more than when it lay loose on its supports. On making the experiment, we had the lath without shoulders laid loose on the frame. It bent like a hoop before it broke with something less than 11 lb. Next we put in the one with shoulders, and drove the wedges very tight: it sustained 240 lb. The Doctor was pleased, and much surprised that my experiment should differ so much from all those published by eminent men. This is accounted for by having shoulders on the ends of the lath, which, butting against the ends of the frame, cannot bend enough to allow the fibres of the wood to form a fulcrum to break one another. This experiment shows how necessary it is that all timbers should be carefully coaked down on plates of buildings, and not dovetailed, as is often done in carpentry. Much timber would be saved, and the building made stronger, by the foregoing plan of shoulders or coaking where it can be applied. The French, in ship-building, are particularly attentive to scoring and coaking wherever they can.

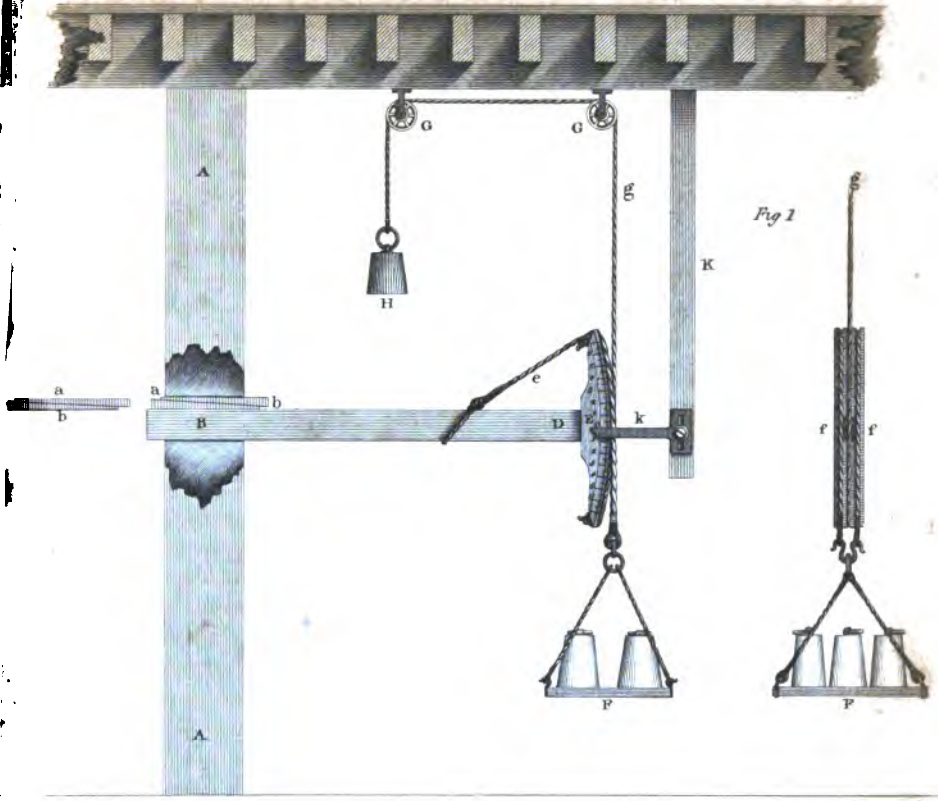


Fig 1

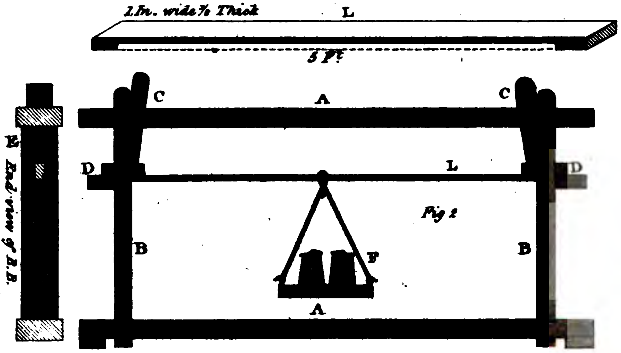


Fig 2

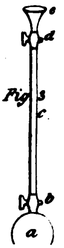


Fig 3

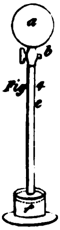
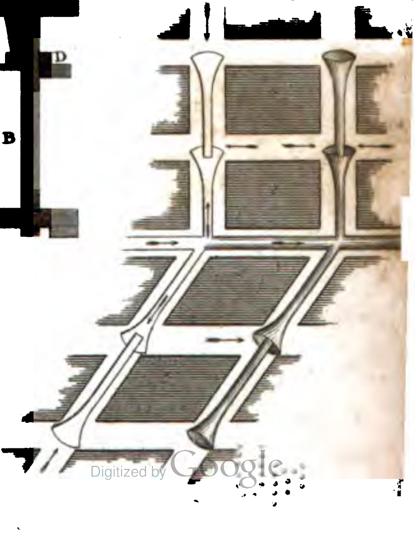


Fig 4

Fig 5



Weight.	Curvature.	Difference.	No. 17.
14 lb	0° 14′	0° 14′	Broke at fulcrum. Length of splinter 20 in.
28	0 30	0 16	
42	0 48	0 18	
56	1 06	0 18	
70	1 24	0 18	
84	1 42	0 18	
98	2 01	0 19	
112	2 24	0 23	
126	2 48	0 24	
140	3 24	0 36	
154	4 12	0 48	
161	4 42	0 30	
168	5 18	0 36	
172	5 42	0 24	
174	6 30	0 48	
106·6 Mean			

Weight.	Curvature.	Difference.	No. 18.
14 lb	0° 12′	0° 12′	Broke at fulcrum. Length of splinter 20 [illegible].
28	0 28	0 16	
42	0 45	0 17	
56	1 02	0 17	
70	1 18	0 16	
84	1 36	0 18	
98	1 57	0 21	
112	2 18	0 21	
[illegible]	2 42	0 24	
140	3 18	0 36	
154	4 00	0 42	
168	4 54	0 54	
175	5 48	0 54	
179	6 12	0 24	
103·3 Mean			

Weight.	Curvature.	Difference.	No. 19.
14 lb	0° 12′	0° 12′	Broke at fulcrum.
28	0 25	0 13	
42	0 39	0 14	
56	0 54	0 15	
70	1 09	0 15	
84	1 24	0 15	
98	1 40	0 16	
112	1 58	0 18	
126	2 18	0 20	
140	2 36	0 18	
154	3 06	0 30	
168	3 42	0 36	
172	4 30	0 48	
179	5 00	0 30	
183	5 24	0 24	
185	5 38	0 14	
188	6 12	0 34	
190	6 18	0 06	
192	6 30	0 12	
196	7 30	1 00	
203	8 18	0 48	
214	0 00	0 42	
136·1 Mean			

Weight.	Curvature.	Difference.	No. 20.
14 lb	0° 12′	0° 12′	Broke 1 in. from fulcrum.
28	0 26	0 14	
42	0 42	0 16	
56	0 57	0 15	
70	1 14	0 17	
84	1 32	0 18	
98	1 54	0 22	
112	2 18	0 24	
126	2 42	0 24	
133	0 00	0 00	
76·3 Mean			

Weight.	Curvature.	Difference.	No. 21.
14 lb	0° 16′	0° 16′	Broke at fulcrum. Length of splinter 10 in.
28	0 34	0 18	
42	0 54	0 20	
56	1 12	0 18	
70	1 33	0 21	
84	1 56	0 23	
98	2 19	0 23	
112	2 48	0 29	
126	3 30	0 42	
140	4 24	0 54	
154	6 12	I 48	
161	7 00	0 48	
90·4 Mean			

Weight.	Curvature.	Difference.	No. 22.
14 lb	0° 15′	0° 15′	Broke at fulcrum. Length of splinter 8½ in.
28	0 30	0 15	
42	0 45	0 15	
56	1 02	0 17	
70	1 18	0 16	
84	1 37	0 19	
98	1 57	0 20	
112	2 19	0 22	
126	2 51	0 32	
140	2 30	0 39	
154	4 30	2 00	
161	5 24	0 54	
90·4 Mean			

Weight.	Curvature.	Difference.	No. 23.
14 lb	0° 18′	0° 18′	Broke at 5 in. from fulcrum. Length of the splinter 15 in.
28	0 39	0 21	
42	0 57	0 18	
56	1 18	0 21	
70	1 42	0 24	
84	2 08	0 26	
98	2 36	0 28	
112	3 09	0 33	
126	4 06	0 57	
140	5 00	0 54	
147	5 30	0 30	
83·4 Mean			

Weight.	Curvature.	Difference.	No. 24.
14 lb	0° 16′	0° 16′	Broke at a knot at the fulcrum. Length of the splinter 15 in.
28	0 38	0 22	
42	0 55	0 17	
56	1 16	0 21	
70	1 37	0 21	
84	2 03	0 26	
98	2 30	0 27	
112	3 06	0 36	
126	4 00	0 54	
133	4 24	0 24	
76·3 Mean			

Weight.	Curvature.	Difference.	No. 25.
14 lb	0° 19′	0° 19′	Broke short at fulcrum.
28	0 44	0 25	
42	1 10	0 26	
56	1 36	0 26	
70	2 06	0 30	
84	2 36	0 30	
98	3 18	0 42	
105	3 42	0 24	
112	4 07	0 25	
116	4 30	0 23	
119	4 48	0 18	
123	5 00	0 12	
126	5 24	0 24	
130	5 48	0 24	
87·3 Mean			

Section of the piece of timber out of which the 25 pieces were cut, and the mean weight each piece bore.

1 107	2 110·5	3 105·6	4 91	5 89
10 90·4	9 101·9	8 122·1	7 118·3	6 109·6
11 108·4	12 128·2	13 Heart. 84	14 97·5	15 82·1
20 63·3	19 106·6	18 103·3	17 136·1	16 76·3
21 90·4	22 90·4	23 83·4	24 76·3	25 87·3

Average weight each piece would sustain 98·4 lb.

Mean of the greatest weight the pieces bore, 167·52 lb.

Six Experiments on Timber spliced three different Ways. Length five Feet, and two Inches square. Splice 12 Inches long, and 13 Inches from the End.

Weight.	Curvature.	Difference.	No. 1.	Weight.	Curvature.	Difference.	No. 2.
14 lb	0° 15′	0° 15′	Splice up and down. Broke in splice.	14b	0° 14′	0° 14′	Splice up and down. Broke in the splice.
28	0 36	0 21		28	0 34	0 20	
42	1 00	0 24		42	0 54	0 20	
56	1 24	0 24		56	1 16	0 22	
70	1 50	0 26		70	1 40	0 24	
84	2 21	0 31		84	2 08	0 28	
98	3 00	0 39		98	2 36	0 28	
105	3 20	0 20		105	2 52	0 16	
112	4 00	0 40		109	0 00	0 00	
67·7 Mean				67·3 Mean			

Weight.	Curvature.	Difference.	No. 3.	Weight.	Curvature.	Difference.	No. 4.
14 lb	0° 24′	0° 24′	Splice flat on the under side, large end uppermost, and near the fulcrum. Nails drew through the small end of the splice.	14 lb	0° 18′	0° 18′	Splice as No. 3, and nails drew through the small end of the splice.
28	0 49	0 25		28	0 48	0 30	
42	1 24	0 35		42	1 16	0 28	
56	2 03	0 39		56	2 00	0 44	
63	2 25	0 22		70	3 00	1 00	
74	2 48	0 23		77	3 30	0 30	
77	3 12	0 24		84	4 12	0 42	
84	3 48	0 36		91	5 00	0 48	
91	4 33	0 45		98	5 24	0 24	
95	5 00	0 27					
97	5 15	0 15		62·2 Mean			
100	5 39	0 24					
104	6 18	0 39					
70·9 Mean							

Weight.	Curvature.	Difference.	No. 5.	Weight.	Curvature.	Difference.	No. 6.
14 lb	0° 20′	0° 20′	Splice thin end uppermost, and next the fulcrum. Broke in thick part of splice, about 1½ in. from fulcrum.	14	0° 20′	0° 20′	Broke in thick part of splice, 5 in. from fulcrum.
28	0 48	0 28		28	0 54	0 34	
42	1 18	0 30		42	1 36	0 42	
56	1 40	0 22		56	2 18	0 42	
70	2 42	0 02		70	3 12	0 54	
84	3 39	0 57		84	4 30	1 18	
				88	5 28	0 58	
49 Mean				90	0 00	0 00	
				59 Mean			

Six Pieces of English Oak, each Piece five Feet long, and two Inches square.

Weight.	Curvature.	Difference.	No. 1.
14 lb	0° 9′	0° 9′	Broke at fulcrum. Length of splinter 18 in.
28	0 21	0 12	
42	0 30	0 09	
56	0 42	0 12	
70	0 54	0 12	
84	1 04	0 10	
98	1 15	0 11	
112	1 27	0 12	
126	1 38	0 11	
140	1 52	0 14	
154	2 04	0 12	
168	2 18	0 14	
182	2 33	0 15	
196	2 54	0 21	
210	3 13	0 19	
217	3 28	0 15	
224	3 38	0 10	
238	5 00	0 22	
245	5 30	0 30	
259	0 00	0 00	
266	0 00	0 00	
148·9 Mean			

Weight.	Curvature.	Difference.	No. 2.
14	0° 7′	0° 7′	Split in the middle.
28	0 18	0 11	
42	0 28	0 10	
56	0 39	0 11	
70	0 51	0 12	
84	1 03	0 12	
98	1 14	0 11	
112	1 26	0 12	
126	1 39	0 13	
140	1 52	0 13	
154	2 06	0 14	
168	2 20	0 14	
182	2 36	0 16	
196	2 54	0 18	
210	3 18	0 24	
224	3 42	0 24	
238	4 12	0 30	
252	4 48	0 36	
266	6 00	0 12	
273	6 42	0 42	
146·6 Mean			

Weight.	Curvature.	Difference.	No. 3.
14 lb	0° 7′	0° 7′	Broke at fulcrum. Length of splinter 2 feet.
28	0 18	0 11	
42	0 27	0 9	
56	0 39	0 12	
70	0 50	0 11	
84	1 01	0 11	
98	1 12	0 11	
112	1 24	0 12	
126	1 38	0 14	
140	1 52	0 14	
154	2 06	0 14	
168	2 21	0 15	
182	2 36	0 15	
196	2 57	0 21	
210	3 20	0 23	
224	0 00	0 00	
119 Mean			

Weight.	Curvature.	Difference.	No. 4.
14 lb	0° 9′	0° 9′	Broke short at fulcrum, a knot.
28	0 18	0 9	
42	0 27	0 9	
56	0 39	0 12	
70	0 49	0 10	
84	1 00	0 11	
98	1 10	0 10	
112	1 22	0 12	
126	1 33	0 11	
140	1 46	0 13	
154	1 58	0 12	
168	2 12	0 14	
182	2 26	0 14	
196	2 42	0 16	
210	3 01	0 19	
224	3 18	0 17	
231	3 36	0 18	
2	3 48	0 12	
245	4 00	0 12	
252	4 18	0 18	
259	4 37	0 19	
266	4 54	0 17	
270	5 12	0 18	
273	5 30	0 30	
277	5 46	0 16	
280	6 12	0 26	
284	7 00	0 48	
177 Mean			

Weight.	Curvature.	Difference.	No. 5.	Weight	Curvature.	Difference.	No. 6.
14 lb	0° 10′	0° 10′	Broke 9 in. from fulcrum. A knot. Splinter 5 in. The grain reversed.	14 lb	0° 7′	0° 7	Broke short 2½ in. from fulcrum. A knot.
28	0 21	0 11		28	0 18	0 11	
42	0 32	0 11		42	0 27	0 9	
56	0 44	0 12		56	0 40	0 13	
70	0 56	0 12		70	0 51	0 11	
84	1 08	0 12		84	1 03	0 12	
98	1 19	0 11		98	1 14	0 11	
112	1 34	0 15		112	1 26	0 12	
126	1 48	0 14		126	1 39	0 13	
140	2 00	0 12		140	1 51	0 12	
154	2 18	0 18		154	2 06	0 15	
168	2 31	0 13		168	2 20	0 14	
182	2 54	0 23		182	2 36	0 16	
196	3 15	0 21		196	2 54	0 18	
210	3 45	0 30		210	3 15	0 21	
224	4 30	0 45		224	3 42	0 27	
231	5 00	0 30		238	4 15	0 33	
125·6 Mean				245	4 36	0 21	
				252	5 00	0 24	
				259	5 18	0 18	
				266	5 54	0 36	
				273	6 54	1 00	
				156·2 Mean			

Average strength of the whole piece, 145·5 lb.
Mean of the greatest weight the pieces bore, 258·5 lb.

A Piece of Riga Fir cut into 26 Pieces, each Piece five Feet long, and two Inches square.

Weight.	Curvature.	Difference.	No. 1.
14 lb	0° 8'	0° 8'	Broke short at fulcrum.
28	0 18	0 10	
42	0 30	0 12	
56	0 42	0 12	
70	0 56	0 14	
84	1 10	0 14	
98	1 24	0 14	
112	1 39	0 15	
126	1 55	0 16	
140	2 18	0 23	
154	2 48	0 30	
168	3 48	0 00	
182	0 00	1 00	
98·1	Mean		

Weight.	Curvature.	Difference.	No. 2.
14 lb	0° 8'	0° 8'	Broke at fulcrum.
28	0 18	0 10	
42	0 30	0 12	
56	0 42	0 12	
70	0 55	0 13	
84	1 08	0 13	
98	1 21	0 13	
112	1 36	0 15	
126	1 54	0 18	
140	2 16	0 22	
154	2 48	0 32	
168	4 06	0 18	
175	5 06	1 00	
97·5	Mean		

Weight.	Curvature.	Difference.	No. 3.
14 lb	0° 6'	0° 6'	Broke short at fulcrum.
28	0 17	0 11	
42	0 26	0 9	
56	0 39	0 13	
70	0 49	0 10	
84	1 02	0 13	
98	1 14	0 12	
112	1 30	0 16	
126	1 46	0 16	
140	2 06	0 20	
154	2 36	0 30	
168	3 24	0 48	
182	4 42	0 18	
98	Mean		

Weight.	Curvature.	Difference.	No. 4.
14 lb	0° 6'	0° 6'	Broke short at fulcrum.
28	0 16	0 10	
42	0 26	0 10	
56	0 36	0 10	
70	0 48	0 12	
84	0 59	0 11	
98	1 12	0 13	
112	1 24	0 12	
126	1 40	0 16	
140	2 00	0 20	
154	2 30	0 30	
168	3 30	1 00	
175	4 30	1 00	
182	5 48	1 18	
103·5	Mean		

Mr. Seppings, the Surveyor of the Navy, has applied the principle with great advantage to framing the decks of ships; and, were it applied to the outside planking, would be of great advantage by taking off the strain, and working from the treenails. To return to my lath, I tried the experiment as to expansion, I marked two fine lines on the lath at four feet distance from each other, and marked them off on a table. I put the lath into the frame, with the wedges driven in tight; kept it in that state some weeks, in order to try if it were possible to extend it. On comparing it with the lines made on the table, when taken out, there was not the least alteration."

I hope these experiments may induce some of your scientific readers to throw more light on a subject not sufficiently understood, and with great regard believe me,

My dear Sir, very sincerely yours,
MARK BEAUFOY.

Experiments on the Strength of a square Piece of Dantzic Oak cut into 25 Pieces, each Piece five Feet long, and two Inches square.

Weight.	Curvature.	Difference.	No. 1.
14 lb	0° 00′	0° 00′	Broke at the fulcrum. Length of the splinter 2 inches.
28	0 00	0 00	
42	0 48	0 48	
56	1 10	0 22	
70	1 24	0 14	
84	1 42	0 18	
98	2 03	0 21	
112	2 23	0 20	
126	2 40	0 17	
140	3 00	0 20	
154	3 40	0 40	
168	4 16	0 36	
182	5 00	0 44	
196	6 12	1 12	
107	Mean		

Weight.	Curvature.	Difference.	No. 2.
14 lb	0° 12′	0° 12′	Broke at a knot 10¼ from the fulcrum. Length of the splinter 2 inches.
28	0 30	0 18	
42	0 50	0 20	
56	1 08	0 18	
70	1 26	0 18	
84	1 48	0 22	
98	2 06	0 18	
112	2 27	0 21	
126	2 54	0 27	
140	3 21	0 27	
154	3 52	0 31	
168	4 43	0 51	
182	6 00	1 17	
189	6 42	0 42	
193	7 00	0 18	
110·5	Mean		

Weight.	Curvature.	Difference.	No. 3.
14 lb	0° 16′	0° 16′	Broke at a knot. Length of the splinter 6 inches.
28	0 35	0 19	
42	0 54	0 19	
56	1 15	0 21	
70	1 36	0 21	
84	1 57	0 21	
98	2 21	0 24	
112	2 48	0 27	
126	3 18	0 30	
140	3 56	0 38	
154	4 54	0 58	
168	6 12	1 18	
105·6	Mean		

Weight.	Curvature.	Difference.	No. 4.
14 lb	0° 16′	0° 16′	Broke 5 in. from the fulcrum. Length of the splinter 12¼ in.
28	0 35	6 19	
48	1 02	0 27	
56	1 24	0 22	
70	1 46	0 22	
84	2 10	0 24	
98	2 36	0 26	
112	3 06	0 30	
126	3 42	0 36	
140	4 26	0 44	
154	5 32	1 06	
168	7 12	1 40	
91	Mean		

Weight.	Curvature.	Difference.	No. 5.
14 lb	0° 20′	0° 20′	Broke 3½ in. from the fulcrum. Length of the splinter 6 in.
28	0 39	0 19	
42	1 07	0 28	
56	1 30	0 23	
70	1 54	0 24	
84	2 18	0 24	
98	0 48	0 30	
112	3 21	0 33	
126	4 00	0 39	
140	5 00	1 00	
147	5 32	0 32	
151	5 54	0 22	
89	Mean		

Weight.	Curvature.	Difference.	No. 6.
14 lb	0° 15′	0° 15′	Broke at fulcrum. Length of the splinter 11¾ in. and 1 in. within the splinter a knot on the under side.
28	0 33	0 18	
42	0 59	0 26	
56	1 20	0 21	
70	1 43	0 23	
84	2 06	0 23	
98	2 32	0 26	
112	3 01	0 29	
126	3 37	0 36	
140	4 33	0 56	
154	5 48	1 15	
161	6 12	0 24	
90·4	Mean		

Weight.	Curvature.	Difference.	No. 7.
14 lb	0° 15′	0° 15′	Broke at fulcrum. Length of the splinter 6 in.
28	0 33	0 18	
42	0 58	0 25	
56	1 18	0 20	
70	1 39	0 21	
84	2 01	0 22	
98	2 24	0 23	
112	2 48	0 24	
126	3 22	0 34	
140	3 57	0 35	
154	4 50	0 53	
161	5 30	0 40	
165	5 54	0 24	
175	7 12	1 18	
101·9 Mean			

Weight.	Curvature.	Difference.	No. 8.
14 lb	0° 18′	0° 18′	Broke at fulcrum. Length of the splinter 14 in.
28	0 36	0 18	
42	0 54	0 18	
56	1 12	0 18	
70	1 30	0 18	
84	1 49	0 19	
98	2 09	0 20	
112	2 30	0 21	
126	2 54	0 24	
140	3 24	0 30	
154	4 00	0 36	
161	4 18	0 18	
168	4 48	0 30	
172	5 06	0 18	
174	5 18	0 12	
177	5 36	0 18	
179	5 48	0 12	
181	5 53	0 05	
184	6 15	0 22	
122·1 Mean			

Weight.	Curvature.	Difference.	No. 9.
14 lb	0° 15′	0° 15′	Broke at fulcrum.
28	0 30	0 15	
42	0 45	0 15	
56	1 02	0 17	
70	1 18	0 16	
84	1 35	0 17	
98	1 54	0 19	
112	2 12	0 18	
126	2 30	0 18	
140	2 54	0 24	
154	3 24	0 30	
168	3 52	0 28	
172	4 44	0 52	
179	5 06	0 22	
186	5 36	0 30	
190	6 06	0 30	
192	6 30	0 24	
118·3 Mean			

Weight.	Curvature.	Difference.	No. 10.
14 lb	0° 12′	0° 12′	Broke at fulcrum. Length of the splinter 1 in.
28	0 27	0 15	
42	0 42	0 15	
56	0 57	0 15	
70	1 12	0 15	
84	1 28	0 16	
98	1 46	0 18	
112	2 03	0 17	
126	2 18	0 15	
140	2 48	0 30	
154	3 07	0 19	
168	3 34	0 27	
172	4 12	0 38	
186	5 00	0 48	
193	5 25	0 25	
109·6 Mean			

Weight.	Curvature.	Difference.	No. 11.
14 lb	0° 15′	0° 15′	Broke at fulcrum. Length of the splinter 10¾ in.
28	0 30	0 15	
42	0 45	0 15	
56	1 03	0 18	
70	1 21	0 18	
84	1 39	0 18	
98	1 58	0 19	
112	2 19	0 21	
126	2 42	0 23	
140	3 12	0 30	
154	3 42	0 30	
168	4 18	0 36	
172	5 18	1 00	
179	5 54	0 36	
183	0 00	0 00	
108·4	Mean		

Weight.	Curvature.	Difference.	No. 12.
14 lb	0° 15′	0° 15′	Split at the fulcrum, and continued to the end of the piece, 12 in.
28	0 30	0 15	
42	0 45	0 15	
56	1 01	0 16	
70	1 17	0 16	
84	1 33	0 16	
98	1 51	0 18	
112	2 10	0 19	
126	2 31	0 21	
140	2 56	0 25	
154	3 30	0 34	
168	4 12	0 42	
182	5 27	0 15	
186	5 54	0 27	
189	6 18	0 24	
193	6 48	0 30	
195	7 12	0 24	
198	8 00	0 48	
202	9 00	1 00	
128·2	Mean		

Weight.	Curvature.	Difference.	No. 13.
14 lb	0° 15′	0° 15′	Heart of the tree. Shakey and broke in continuation of heart shake.
28	0 34	0 19	
42	0 52	0 18	
56	1 12	0 20	
70	1 33	0 21	
84	1 56	0 23	
98	2 24	0 28	
112	2 55	0 31	
126	3 44	0 49	
140	4 45	0 01	
154	6 00	0 15	
84	Mean		

Weight.	Curvature.	Difference.	No. 14.
14 lb	0° 18′	0° 18′	Broke at fulcrum. Length of the splinter 19 in.
28	0 35	0 17	
42	0 50	0 15	
56	1 09	0 19	
70	1 27	0 18	
84	1 47	0 20	
98	2 04	0 17	
112	2 27	0 23	
126	2 52	0 25	
140	3 19	0 27	
154	3 54	0 35	
168	4 54	1 00	
175	0 00	0 00	
97·5	Mean		

Weight.	Curvature.	Difference.	No. 15.
14 lb	0° 15′	0° 15′	Broke 8 in. from fulcrum at a knot. Length of splinter 14 in.
28	0 36	0 21	
42	0 56	0 20	
56	1 15	0 19	
70	1 36	0 21	
84	1 57	0 21	
98	2 23	0 26	
112	2 45	0 22	
126	3 19	0 34	
133	3 36	0 17	
140	3 54	0 18	
82·1	Mean		

Weight.	Curvature.	Difference.	No. 16.
14 lb	0° 15′	0° 15′	Broke 1 in. from fulcrum at a shake. Length of the splinter 20 in.
28	0 36	0 21	
42	0 58	0 22	
56	1 20	0 22	
70	1 44	0 24	
84	2 10	0 26	
98	2 37	0 27	
114	2 54	0 27	
63·5	Mean		

Weight.	Curvature.	Difference.	No. 5.
14 lb	0° 09′	0° 09′	Broke at fulcrum. Splinter 5¼ in.
28	0 19	0 10	
42	0 32	0 13	
56	0 44	0 12	
70	0 58	0 14	
84	1 12	0 14	
98	1 23	0 11	
112	1 36	0 13	
126	1 49	0 13	
140	2 03	0 14	
154	2 18	0 15	
168	2 36	0 18	
182	2 48	0 12	
189	2 58	0 10	
196	3 06	0 08	
203	3 18	0 12	
210	3 36	0 18	
217	3 48	0 12	
224	4 00	0 12	
231	4 38	0 38	
238	5 00	0 22	
142	Mean		

Weight.	Curvature.	Difference.	No. 6.
14 lb	0° 10′	0° 10′	Broke at fulcrum. Splinter 4 in. long.
28	0 20	0 10	
42	0 32	0 12	
56	0 43	0 11	
70	0 58	0 15	
84	1 12	0 14	
98	1 26	0 14	
112	1 43	0 17	
126	2 00	0 17	
140	2 28	0 28	
154	2 42	0 14	
168	3 00	0 18	
91	Mean		

Weight.	Curvature.	Difference.	No. 7.
14 lb	0° 12′	0° 12′	Broke at a knot 2 in. from the fulcrum. Splinter 12 in. long.
28	0 24	0 12	
42	0 36	0 12	
56	0 52	0 16	
70	1 04	0 12	
84	1 20	0 16	
98	1 36	0 16	
112	1 54	0 18	
126	2 12	0 18	
140	2 30	0 18	
154	3 00	0 30	
161	3 12	0 12	
168	3 36	0 24	
175	4 00	0 24	
182	4 06	0 06	
107·3	Mean		

Weight.	Curvature.	Difference.	No. 8.
14 lb	0° 09′	0° 09′	Broke at fulcrum. Splinter 6 in.
28	0 21	0 12	
42	0 33	0 12	
56	0 45	0 12	
70	0 58	0 13	
84	1 10	0 12	
98	1 25	0 15	
112	1 40	0 15	
126	1 56	0 16	
140	2 14	0 18	
154	2 36	0 22	
161	2 48	0 22	
168	3 00	0 22	
175	3 24	0 24	
182	3 54	0 30	
189	4 24	0 30	
196	5 00	0 36	
203	5 30	0 30	
122·1	Mean		

Weight.	Curvature.	Difference.	No. 9.
14 lb	0° 10′	0° 10′	Broke short at fulcrum.
28	0 21	0 11	
42	0 33	0 12	
56	0 44	0 11	
70	0 56	0 12	
84	1 08	0 12	
98	1 20	0 12	
112	1 32	0 12	
126	1 45	0 12	
140	1 58	0 13	
154	2 13	0 15	
168	2 30	0 17	
175	2 42	0 12	
182	2 54	0 12	
189	3 03	0 09	
196	3 08	0 05	
203	3 30	0 22	
210	3 48	0 18	
217	4 12	0 24	
224	4 36	0 24	
231	5 30	0 54	
238	6 00	0 30	
143·5	Mean		

Weight.	Curvature.	Difference.	No. 10.
14 lb	0° 06′	0° 06′	Broke short at fulcrum.
28	0 20	0 14	
42	0 30	0 10	
56	0 42	0 12	
70	0 54	0 12	
84	1 04	0 10	
98	1 16	0 12	
112	1 28	0 12	
126	1 42	0 14	
140	1 55	0 13	
154	2 08	0 13	
168	2 24	0 16	
182	2 42	0 18	
189	2 50	0 08	
196	3 12	0 22	
203	3 26	0 14	
210	0 00	0 00	
217	3 42	0 16	
224	3 54	0 12	
231	4 12	0 28	
238	4 36	0 24	
245	5 12	0 36	
252	6 24	0 12	

Weight.	Curvature.	Difference.	No. 11.
14 lb	0° 10′	0° 10′	Broke at fulcrum. Splinter 6 in.
28	0 22	0 12	
42	0 34	0 12	
56	0 48	0 14	
70	1 00	0 12	
84	1 12	0 12	
98	1 26	0 14	
112	1 39	0 13	
126	1 54	0 15	
140	2 10	0 16	
154	2 27	0 17	
168	2 48	0 21	
182	3 09	0 21	
196	3 42	0 33	
203	4 00	0 18	
210	4 36	0 36	
217	5 30	0 54	
123·5	Mean		

Weight.	Curvature.	Difference.	No. 12.
14 lb	0° 10′	0° 10′	Broke at a knot 2½ in. from fulcrum.
28	0 22	0 12	
42	0 36	0 14	
56	0 50	0 14	
70	1 04	0 14	
84	1 20	0 16	
98	1 39	0 19	
112	1 57	0 18	
126	2 18	0 21	
140	2 36	0 18	
154	3 06	0 30	
168	3 50	0 44	
91	Mean		

Weight.	Curvature.	Difference.	No. 13, Heart.
14 lb	0° 17′	0° 17′	Broke at some knots 2 in. from fulcrum.
28	0 36	0 19	
42	0 52	0 16	
56	1 14	0 22	
70	1 36	0 22	
84	2 06	0 30	
98	2 30	0 24	
56	Mean		

Weight.	Curvature.	Difference.	No. 14.
14 lb	0° 10′	0° 10′	Broke at a knot 2¼ in. from fulcrum. Splinter 3 in.
28	0 21	0 11	
42	0 33	0 12	
56	0 46	0 13	
70	0 58	0 12	
84	1 12	0 14	
98	1 26	0 14	
112	1 44	0 18	
126	2 00	0 16	
140	2 24	0 24	
154	2 48	0 24	
168	3 36	0 48	
182	4 30	0 54	
98	Mean		

Weight.	Curvature.	Difference.	No. 15.
14 lb	0° 10′	0° 10′	Broke at fulcrum almost short.
28	0 21	0 11	
42	0 32	0 11	
56	0 45	0 13	
70	0 56	0 11	
84	1 08	0 12	
98	1 21	0 13	
112	1 36	0 15	
126	1 48	0 12	
140	2 06	0 18	
154	2 21	0 15	
168	2 42	0 21	
182	3 12	0 30	
189	3 24	0 12	
196	3 46	0 22	
203	4 12	0 26	
210	4 30	0 18	
121·9	Mean		

Weight.	Curvature.	Difference.	No. 16.
14 lb	0° 10′	0° 10′	Broke short at a knot 4 in. from fulcrum.
28	0 20	0 10	
42	0 30	0 10	
56	0 40	0 10	
70	0 50	0 10	
84	1 04	0 14	
98	1 14	0 10	
112	1 26	0 12	
126	1 36	0 10	
140	1 50	0 14	
154	2 03	0 13	
168	2 18	0 15	
182	2 34	0 16	
189	2 42	0 08	
196	2 54	0 12	
203	3 03	0 09	
210	3 12	0 09	
217	3 24	0 12	
224	3 42	0 18	
231	4 00	0 18	
238	4 30	0 30	
245	5 00	0 30	
252	6 12	0 12	
151·3	Mean		

Weight.	Curvature.	Difference.	No. 17.	Weight.	Curvature.	Difference.	No. 18.
14 lb	0° 09′	0° 09′	Broke short 4 in. from fulcrum.	14 lb	0° 10′	0° 10′	Broke at a knot 4 in. from fulcrum.
28	0 20	0 11		28	0 21	0 11	
42	0 33	0 13		42	0 34	0 13	
56	0 44	0 11		56	0 48	0 14	
70	0 57	0 13		70	1 00	0 12	
84	1 08	0 11		84	1 15	0 15	
98	1 20	0 12		98	1 30	0 15	
112	1 34	0 14		112	1 48	0 18	
126	1 54	0 20		126	2 06	0 18	
140	2 04	0 10		140	2 26	0 20	
154	2 21	0 17		154	3 03	0 37	
168	2 42	0 21		161	4 00	0 57	
182	3 06	0 24					
189	3 36	0 30		90·4 Mean			
104·5 Mean							

Weight.	Curvature.	Difference.	No. 19.	Weight.	Curvature.	Difference.	No. 20.
14 lb	0° 10′	0° 10′	Broke at a knot 4 in. from fulcrum. Splinter 4 in. long.	14 lb	0° 12′	0° 12′	Broke short at fulcrum.
28	0 24	0 12		28	0 24	0 12	
42	0 38	0 14		42	0 36	0 12	
56	0 54	0 16		56	0 48	0 12	
70	1 07	0 17		70	1 03	0 15	
80	1 24	0 17		84	1 18	0 15	
98	1 37	0 13		98	1 31	0 13	
112	1 56	0 19		112	1 49	0 18	
126	2 23	0 27		126	2 06	0 17	
140	2 54	0 31		140	2 27	0 21	
147	4 00	0 06		154	2 48	0 21	
154	4 36	0 36		161	3 06	0 18	
				168	3 18	0 12	
88·9 Mean				175	3 38	0 20	
				182	3 48	0 10	
				189	4 06	0 18	
				196	4 30	0 24	
				203	5 00	0 30	
				210	0 00	0 00	
				126·7 Mean			

Weight.	Curvature.	Difference.	No. 21.
14 lb	0° 10′	0° 10′	Broke short at fulcrum.
28	0 21	0 11	
42	0 32	0 11	
56	0 44	0 12	
70	0 55	0 11	
84	1 08	0 13	
98	1 20	0 12	
112	1 34	0 14	
126	1 47	0 13	
140	2 00	0 13	
154	2 16	0 16	
168	2 32	0 16	
175	2 42	0 10	
182	2 54	0 12	
189	3 06	0 12	
196	3 18	0 12	
203	3 36	0 18	
210	3 54	0 18	
217	4 12	0 18	
224	4 36	0 24	
231	4 49	0 13	
238	6 12	1 23	
143	Mean		

Weight.	Curvature.	Difference.	No. 22.
14 lb	0° 09′	0° 09′	Broke short at fulcrum.
28	0 20	0 11	
42	0 30	0 10	
56	0 41	0 11	
70	0 53	0 12	
84	1 05	0 12	
98	1 18	0 13	
112	1 30	0 12	
126	1 43	0 13	
140	2 00	0 17	
147	2 11	0 11	
154	2 18	0 07	
161	2 30	0 12	
168	2 39	0 09	
175	2 48	0 09	
182	3 00	0 12	
189	3 12	0 12	
196	3 24	0 12	
203	3 42	0 18	
210	4 00	0 18	
217	4 24	0 24	
224	4 48	0 24	
136·2	Mean		

Weight.	Curvature.	Difference.	No. 23.
14 lb	0° 12′	0° 12′	Broke short at fulcrum.
28	0 21	0 09	
42	0 32	0 11	
56	0 42	0 10	
70	0 54	0 12	
84	1 05	0 11	
98	1 16	0 11	
112	1 28	0 12	
126	1 42	0 14	
140	1 55	0 13	
152	2 12	0 17	
168	2 30	0 18	
175	2 39	0 09	
182	2 54	0 15	
189	3 03	0 09	
196	3 18	0 15	
203	3 33	0 15	
210	3 42	0 09	
217	4 00	0 18	
224	4 18	0 18	
231	5 00	0 42	
238	5 48	0 48	
143·5	Mean		

Weight.	Curvature.	Difference.	No. 24.
14 lb	0° 12′	0° 12′	Broke 4 in. from the fulcrum, at a knot. Splinter 4 in.
28	0 24	0 12	
42	0 35	0 11	
56	0 48	0 13	
70	0 59	0 11	
84	1 10	0 11	
98	1 24	0 14	
112	1 38	0 14	
126	1 52	0 14	
140	2 08	0 16	
154	2 24	0 16	
168	2 48	0 24	
175	3 00	0 12	
97·6	Mean		

Weight.	Curvature.	Difference.	No. 25.
14 lb	0° 10′	0° 10′	Broke short at fulcrum.
28	0 20	0 10	
42	0 30	0 10	
56	0 42	0 12	
70	0 54	0 12	
84	1 04	0 10	
98	1 16	0 12	
112	1 27	0 11	
126	1 40	0 13	
140	1 54	0 14	
154	2 06	0 12	
168	2 24	0 18	
175	2 33	0 09	
182	2 42	0 09	
189	2 54	0 12	
196	3 09	0 15	
203	3 21	0 12	
210	3 37	0 16	
217	3 54	0 17	
224	4 12	0 18	
231	4 36	0 24	
238	5 18	0 42	
245	0 00	0 00	
147·8 Mean			

Weight.	Curvature.	Difference.	No. 26.
14 lb	0° 09′	0° 09′	Broke at fulcrum.
28	0 20	0 11	
42	0 30	0 10	
56	0 40	0 10	
70	0 52	0 12	
84	1 04	0 12	
98	1 15	0 11	
112	1 26	0 11	
126	1 39	0 13	
140	1 50	0 11	
154	2 06	0 16	
168	2 21	0 15	
182	2 26	0 15	
189	2 45	0 09	
196	3 00	0 15	
203	3 08	0 08	
210	3 18	0 10	
217	3 36	0 18	
224	3 54	0 18	
231	4 00	0 00	
137·2 Mean			

Section of the Piece of Riga Fir out of which the 26 Pieces were cut, and the Mean Weight each Piece sustained.

	23 143·5	24 97·6	25 147·8	26 137·2	
17 104·5	18 90·4	19 88·9	20 126·7	21 143	22 136·2
11 123·5	12 91	13 Heart. 56	14 98	15 121·9	16 151·3
5 142	6 91	7 107·3	8 122·1	9 143·5	10 155·7
	1 98·1	2 97·5	3 98	4 103·5	

The average weight each piece would sustain, 116·01 lb.
Mean of the greatest weight the pieces bore, 207·73 lb.

A Piece of English Oak cut into 15 *Pieces, each five Feet long, and two Inches square. This Piece of Wood very irregular and cross grained.*

Weight.	Curvature.	Difference.	No. 1.
14 lb	0° 09′	0° 09′	Broke at fulcrum.
28	0 21	0 12	
42	0 30	0 09	
56	0 42	0 12	
70	0 54	0 12	
84	1 06	0 12	
98	1 17	0 11	
112	1 30	0 13	
126	1 44	0 14	
140	2 02	0 18	
154	2 19	0 17	
168	2 39	0 20	
175	2 54	0 15	
182	3 09	0 15	
189	3 24	0 15	
196	3 48	0 24	
203	4 06	0 18	
210	4 30	0 24	
217	5 18	0 48	
224	6 12	0 54	
231	7 30	0 18	
238	8 42	0 12	
245	0 00	0 00	
147·9 Mean			

Weight.	Curvature.	Difference.	No. 2.
14 lb	0° 10′	0° 10′	
28	0 24	0 12	
42	0 32	0 12	
56	0 42	0 10	
70	0 53	0 11	
84	1 05	0 12	
98	1 16	0 11	
112	1 27	0 11	
126	1 40	0 13	
140	1 54	0 14	
154	2 08	0 14	
168	2 28	0 20	
182	2 54	0 26	
189	3 06	0 12	
196	3 26	0 20	
203	3 42	0 16	
210	4 07	0 25	
217	4 30	0 23	
224	5 30	1 00	
231	6 36	1 06	
238	0 00	0 00	
142 Mean			

Weight.	Curvature.	Difference.	No. 3.
14 lb	0° 12′	0° 12′	
28	0 24	0 12	
42	0 43	0 19	
56	0 54	0 11	
70	1 12	0 18	
84	1 30	0 18	
98	1 54	0 24	
112	2 24	0 30	
126	2 56	0 32	
140	3 30	0 34	
154	4 20	0 50	
168	5 00	0 40	
182	6 00	1 00	
196	7 30	1 30	
203	8 48	1 18	
111·5 Mean			

Weight.	Curvature.	Difference.	No. 4.
14 lb	0° 10′	0° 10′	
28	0 22	0 12	
42	0 36	0 14	
56	0 52	0 16	
70	1 10	0 18	
84	1 30	0 20	
98	1 56	0 26	
112	2 24	0 28	
126	3 00	0 36	
140	3 36	0 36	
154	4 30	0 54	
168	5 30	1 00	
182	7 00	2 00	
189	7 30	0 30	
196	9 00	1 30	
203	0 00	0 00	
210	0 00	0 00	
121·3 Mean			

Weight.	Curvature.	Difference.	No. 5.
14 lb	0° 09′	0° 09′	Broke short at fulcrum.
28	0 19	0 10	
42	0 30	0 11	
56	0 42	0 12	
70	0 54	0 12	
84	1 07	0 13	
98	1 20	0 13	
112	1 33	0 13	
126	1 48	0 15	
140	2 02	0 14	
154	2 20	0 18	
168	2 42	0 22	
182	3 12	0 30	
189	3 36	0 24	
196	4 00	0 24	
203	4 30	0 30	
210	5 50	1 00	
217	6 30	1 00	
224	8 00	1 30	
231	10 12	2 12	
238	0 00	0 00	
142	Mean		

Weight.	Curvature.	Difference.	No. 6.
14 lb	0° 11′	0° 11′	
28	0 22	0 11	
42	0 34	0 12	
56	0 50	0 16	
70	1 04	0 14	
84	1 18	0 14	
98	1 32	0 14	
112	1 48	0 16	
126	2 06	0 18	
140	2 24	0 18	
154	2 42	0 18	
168	3 04	0 22	
182	3 30	0 26	
189	4 00	0 30	
196	4 24	0 24	
203	5 00	0 36	
210	6 00	1 00	
217	7 00	1 00	
224	8 00	1 00	
231	0 00	0 00	
238	0 00	0 00	
142	Mean		

Weight.	Curvature.	Difference.	No. 7.
14 lb	0° 12′	0° 12′	Broke at fulcrum. Splinter 3 in.
28	0 26	0 14	
42	0 44	0 18	
56	1 00	0 16	
70	1 16	0 16	
84	1 32	0 16	
98	1 47	0 15	
112	2 10	0 23	
126	2 28	0 18	
140	2 54	0 26	
154	3 24	0 30	
168	4 00	0 36	
182	4 54	0 54	
189	5 30	0 36	
196	6 00	0 30	
203	0 00	0 00	
210	0 00	0 00	
217	0 00	0 00	
224	0 00	0 00	
132·3	Mean		

Weight.	Curvature.	Difference.	No. 8.
14 lb	0° 09′	0° 09′	Broke short at fulcrum.
28	0 20	0 11	
42	0 33	0 13	
56	0 47	0 14	
70	1 00	0 13	
84	1 13	0 13	
98	1 26	0 13	
112	1 42	0 16	
126	2 00	0 18	
140	2 21	0 21	
154	2 45	0 24	
168	3 21	0 36	
182	4 06	0 45	
189	4 42	0 36	
196	5 30	0 48	
203	6 30	1 00	
210	8 00	1 30	
217	9 00	1 00	
224	0 00	0 00	
231	0 00	0 00	
137·2	Mean		

Weight.	Curvature.	Difference.	No. 9.
14 lb	0° 09′	0° 09′	Split at the fulcrum end.
28	0 20	0 11	
42	0 34	0 14	
56	0 48	0 14	
70	1 02	0 14	
84	1 21	0 19	
98	1 32	0 11	
112	1 49	0 17	
126	2 07	0 18	
140	2 30	0 23	
154	3 02	0 32	
168	0 00	0 00	
182	0 00	0 00	
189	0 00	0 00	
104·5 Mean			

Weight.	Curvature.	Difference.	No. 10.
14 lb	0° 09′	0° 09′	Broke at fulcrum. Splinter 15 in.
28	0 19	0 10	
42	0 30	0 11	
56	0 43	0 13	
70	0 55	0 12	
84	1 07	0 12	
98	1 20	0 19	
112	1 34	0 14	
126	1 49	0 15	
140	2 07	0 18	
154	2 28	0 21	
168	2 50	0 22	
182	3 24	0 34	
189	3 48	0 24	
196	4 06	0 18	
203	4 30	0 24	
210	5 00	0 30	
217	5 42	0 42	
224	6 30	0 48	
231	8 00	1 30	
238	0 00	0 00	
245	0 00	0 00	
116·7 Mean			

Weight.	Curvature.	Difference.	No. 11.
14 lb	0° 09′	0° 09′	Broke at fulcrum.
28	0 21	0 12	
42	0 33	0 12	
56	0 46	0 13	
70	1 00	0 14	
84	1 14	0 14	
98	1 28	0 14	
112	1 42	0 14	
126	2 00	0 16	
140	2 18	0 18	
154	2 42	0 24	
168	3 18	0 36	
182	4 00	0 42	
189	4 18	0 16	
196	5 00	0 42	
203	5 30	0 30	
210	6 30	1 00	
217	8 00	1 30	
224	9 00	1 00	
231	0 00	0 00	
137·2 Mean			

Weight.	Curvature.	Difference.	No. 12.
14 lb	0° 14′	0° 14′	Broke at fulcrum.
28	0 27	0 13	
42	0 42	0 15	
56	0 56	0 14	
70	1 12	0 16	
84	1 27	0 15	
98	1 44	0 17	
112	2 02	0 18	
126	3 22	0 20	
140	3 44	0 22	
154	4 12	0 28	
168	4 48	0 36	
182	4 30	0 42	
196	6 00	0 30	
203	7 00	0 00	
210	8 12	0 12	
217	9 00	0 48	
224	0 00	0 00	
134·4 Mean			

Weight.	Curvature.	Difference.	No. 13.
14 lb	0° 10′	0° 10′	Broke short at fulcrum.
28	0 25	0 15	
42	0 40	0 15	
56	0 53	0 13	
70	1 86	0 13	
84	1 19	0 13	
98	1 34	0 15	
112	1 48	0 14	
126	2 06	0 18	
140	2 25	0 19	
154	2 48	0 23	
168	3 30	0 42	
182	4 12	0 42	
189	4 36	0 24	
196	5 00	0 24	
203	5 30	0 30	
210	6 30	1 00	
217	7 30	1 00	
224	0 00	0 00	
231	0 00	0 00	
238	0 00	0 00	
142	Mean		

Weight.	Curvature.	Difference.	No. 14.
14 lb	0° 10′	0° 10′	Broke short at fulcrum
28	0 21	0 11	
42	0 34	0 13	
56	0 48	0 14	
70	1 00	0 12	
84	1 16	0 16	
98	1 30	0 14	
112	1 50	0 20	
126	2 18	0 28	
140	2 42	0 24	
154	3 30	0 48	
168	4 30	1 00	
182	6 00	1 30	
189	7 06	1 06	
196	9 00	1 54	
203	11 00	2 00	
210	0 00	0 00	
121·9	Mean		

Weight.	Curvature.	Difference.	No. 15.
14 lb	0° 13′	0° 13′	Broke 20 in. from the index end. Splinter 9 in.
28	0 26	0 13	
42	0 42	0 16	
56	0 54	0 12	
70	1 00	0 16	
84	1 27	0 17	
98	1 46	0 19	
112	2 06	0 20	
126	2 36	0 20	
140	3 09	0 33	
154	4 00	0 51	
168	5 00	1 00	
182	0 00	0 00	
98	Mean		

Average weight each piece would sustain, 128·66 lb.
Mean of the greatest weight each piece bore, 223·53 lb.

A Piece of Pitch Pine cut into 25 Pieces, each Piece five Feet long, and two Inches square.

Weight.	Curvature.	Difference.	No. 1.
14 lb	0° 12′	0° 00′	Broke short at fulcrum.
28	0 28	0 16	
42	0 36	0 12	
56	0 42	0 06	
70	0 48	0 06	
84	1 00	0 12	
98	1 15	0 15	
112	1 24	0 09	
126	1 38	0 14	
140	1 48	0 10	
154	2 03	0 15	
168	2 18	0 15	
182	2 36	0 18	
196	2 48	0 12	
210	3 03	0 15	
224	3 24	0 21	
238	3 54	0 30	
252	4 24	0 30	
266	5 00	0 36	
280	6 18	0 18	
287	7 00	0 42	
153·7 Mean			

Weight.	Curvature.	Difference.	No. 2.
14 lb	0° 12′	0° 00′	Broke short at fulcrum.
28	0 30	0 18	
42	0 36	0 18	
56	0 48	0 12	
70	1 00	0 12	
84	1 06	0 06	
98	1 18	0 12	
112	1 30	0 12	
126	1 42	0 12	
140	1 55	0 13	
154	2 12	0 17	
168	2 24	0 12	
182	2 48	0 24	
196	3 00	0 12	
210	3 18	0 18	
224	3 48	0 20	
238	4 24	0 36	
252	5 12	0 48	
259	5 42	0 30	
266	6 24	0 32	
145·8 Mean			

Weight.	Curvature.	Difference.	No. 3.
14 lb	0° 12′	0° 00′	Broke short at fulcrum.
28	0 30	0 18	
42	0 36	0 18	
56	0 48	0 12	
70	1 00	0 12	
84	1 12	0 12	
98	1 20	0 08	
112	1 36	0 16	
126	1 44	0 08	
140	1 54	0 10	
154	2 08	0 14	
168	2 18	0 10	
182	2 33	0 16	
196	2 48	0 15	
210	3 03	0 15	
224	3 20	0 17	
238	3 45	0 25	
254	4 12	0 27	
266	4 48	0 36	
280	5 36	0 48	
147 Mean			

Weight.	Curvature.	Difference.	No. 4.
14 lb	0° 12′	0° 00′	Broke short at fulcrum.
28	0 30	0 18	
42	0 36	0 06	
56	0 42	0 06	
70	0 48	0 06	
84	0 56	0 08	
98	1 06	0 10	
112	1 18	0 12	
126	1 28	0 10	
140	1 38	0 10	
154	1 48	0 10	
168	2 00	0 12	
182	2 12	0 12	
196	2 24	0 12	
210	2 42	0 18	
224	3 03	0 21	
238	3 36	0 33	
252	4 12	0 36	
259	4 50	0 38	
139·5 Mean			

Weight.	Curvature.	Difference.	No. 5.
14 lb	0° 06′	0° 00′	Broke short at fulcrum.
28	0 15	0 09	
42	0 30	0 15	
56	0 36	0 16	
70	0 42	0 06	
84	0 54	0 12	
98	1 08	0 14	
112	1 18	0 10	
126	1 24	0 06	
140	1 34	0 10	
154	1 44	0 10	
168	1 54	0 10	
182	2 08	0 14	
196	2 18	0 10	
210	2 34	0 16	
224	2 54	0 10	
238	3 12	0 18	
252	3 48	0 16	
259	4 18	0 30	
266	4 54	0 36	
270	5 30	0 36	
151·9 Mean			

Weight.	Curvature.	Difference.	No. 6.
14 lb	0° 06′	0° 00′	Broke at fulcrum. Splinter 10 in.
28	0 12	0 06	
42	0 20	0 08	
56	0 30	0 10	
70	0 48	0 10	
84	0 56	0 08	
98	1 06	0 10	
112	1 15	0 09	
126	1 24	0 09	
140	1 34	0 10	
154	1 52	0 18	
168	2 00	0 08	
182	2 12	0 12	
196	2 24	0 12	
210	2 36	0 12	
224	3 00	0 24	
238	3 30	0 30	
252	4 00	0 30	
266	4 54	0 54	
270	5 30	0 36	
274	6 00	0 30	
152·6 Mean			

Weight.	Curvature.	Difference.	No. 7.
14 lb	0° 06′	0° 00′	Broke. Splinter 12 in.
28	0 12	0 06	
42	0 20	0 08	
56	0 30	0 10	
70	0 40	0 10	
84	0 50	0 10	
98	0 58	0 05	
112	1 08	0 10	
126	1 18	0 10	
140	1 28	0 10	
154	1 36	0 08	
168	1 46	0 10	
182	1 56	0 10	
196	2 10	0 14	
210	2 24	0 14	
224	2 36	0 12	
238	2 56	0 20	
252	3 18	0 22	
266	3 48	0 30	
280	4 36	0 48	
287	5 00	0 24	
294	6 00	1 00	
160 Mean			

Weight.	Curvature.	Difference.	No. 8.
14 lb	0° 06′	0° 00′	Broke at the fulcrum. Splinter 6 in.
28	0 18	0 12	
42	0 24	0 16	
56	0 36	0 12	
70	0 48	0 12	
84	0 58	0 10	
98	1 06	0 08	
112	1 15	0 09	
126	1 28	0 13	
140	1 38	0 10	
154	1 50	0 12	
168	2 06	0 16	
182	2 18	0 12	
196	2 36	0 18	
210	2 54	0 18	
224	3 24	0 30	
238	4 00	0 36	
252	5 00	0 00	
259	5 42	0 42	
266	6 30	0 48	
145·9 Mean			

Weight.	Curvature.	Difference.	No. 9.	Weight.	Curvature.	Difference.	No. 10.
14 lb	0° 06′	0° 00′	Broke at the fulcrum. Splinter 12 in. long.	14 lb	0° 10′	0° 00′	Broke short at fulcrum.
28	0 18	0 12		28	0 18	0 08	
42	0 24	0 06		42	0 30	0 12	
56	0 42	0 18		56	0 42	0 12	
70	0 56	0 14		70	0 54	0 12	
84	1 06	0 10		84	1 06	0 12	
98	1 24	0 18		98	1 20	0 14	
112	1 36	0 12		112	1 36	0 16	
126	1 54	0 18		126	1 52	0 16	
140	2 08	0 14		140	2 10	0 18	
154	2 26	0 18		154	2 26	0 16	
168	2 48	0 22		168	2 46	0 20	
182	3 12	0 24		182	3 10	0 24	
196	3 54	0 42		196	3 40	0 30	
203	5 30	0 36		210	4 08	0 28	
104·9 Mean				224	4 54	0 46	
				238	6 00	1 06	
				245	7 30	1 30	
				132·6 Mean			

Weight.	Curvature.	Difference.	No. 11.	Weight.	Curvature.	Difference.	No. 12.
14 lb	0° 10′	0° 00′	Broke short at fulcrum.	14 lb	0° 08′	0° 00′	Broke short at fulcrum
28	0 20	0 10		28	0 20	0 12	
42	0 30	0 10		42	0 30	0 10	
56	0 40	0 10		56	0 40	0 10	
70	0 48	0 08		70	0 50	0 10	
84	0 58	0 10		84	1 00	0 10	
98	1 10	0 12		98	1 08	0 08	
112	1 20	0 10		112	1 18	0 10	
126	1 30	0 10		126	1 26	0 08	
140	1 42	0 12		140	1 38	0 12	
154	1 54	0 12		154	1 50	0 12	
168	2 06	0 12		168	2 06	0 16	
182	2 18	0 12		182	2 18	0 12	
196	2 32	0 14		196	2 36	0 18	
210	2 48	0 16		210	2 56	0 20	
224	3 06	0 18		224	3 18	0 22	
238	3 30	0 24		238	3 36	0 18	
252	3 54	0 24		252	4 12	0 46	
366	4 30	0 36		266	5 06	0 54	
270	5 18	0 48		270	6 30	1 24	
274	6 00	0 42		274	7 42	1 08	
152·6 Mean				152·6 Mean			

Weight.	Curvature.	Difference.	No. 13.
14 lb	0° 06′	0° 00′	Splinter 3 in. long.
28	0 18	0 12	
42	0 30	0 12	
56	0 40	0 10	
70	0 48	0 08	
84	0 52	0 04	
98	1 00	0 08	
112	1 08	0 08	
126	1 18	0 10	
140	1 28	0 10	
154	1 42	0 14	
168	1 51	0 09	
182	2 02	0 11	
196	2 14	0 12	
210	2 30	0 16	
224	2 42	0 12	
238	3 00	0 18	
252	3 15	0 15	
266	3 36	0 21	
280	4 00	0 24	
294	5 00	0 00	
308	5 30	0 30	
315	6 00	0 30	
322	6 30	0 30	
326	7 00	0 30	
179	Mean		

Weight.	Curvature.	Difference.	No. 14.
14 lb	0° 06′	0° 00′	Broke at fulcrum.—N. B. This piece of wood was the same as No. 13, turned end for end.
28	0 18	0 12	
42	0 30	0 12	
56	0 40	0 10	
70	0 48	0 08	
84	0 58	0 10	
98	1 15	0 17	
112	1 28	0 13	
126	1 40	0 12	
140	1 50	0 10	
154	2 06	0 16	
168	2 18	0 12	
182	2 32	0 14	
196	2 48	0 16	
210	3 08	0 20	
224	3 20	0 12	
238	3 48	0 28	
252	4 00	0 12	
266	4 30	0 30	
273	4 54	0 24	
280	5 30	0 36	
153	Mean		

Weight.	Curvature.	Difference.	No. 15.
14 lb	0° 10′	0° 00′	Broke at fulcrum. Splinter 3 in.
28	0 22	0 12	
42	0 30	0 18	
56	0 42	0 12	
70	0 48	0 16	
84	1 00	0 12	
98	1 10	0 10	
112	1 24	0 14	
126	1 36	0 12	
140	1 48	0 12	
154	2 00	0 12	
168	2 15	0 15	
182	2 30	0 15	
196	2 45	0 15	
210	3 06	0 21	
224	3 26	0 20	
238	3 48	0 22	
252	4 18	0 30	
266	5 00	0 42	
280	6 00	1 00	
287	6 30	0 30	
153·7	Mean		

Weight.	Curvature.	Difference.	No. 16.
14 lb	0° 08′	0° 00′	Broke. Splinter 4 in.
28	0 20	0 12	
42	0 30	0 10	
56	0 42	0 12	
70	0 54	0 12	
84	1 08	0 14	
98	1 15	0 07	
112	1 30	0 15	
126	1 42	0 12	
140	1 55	0 13	
154	2 08	0 13	
168	2 25	0 17	
182	2 50	0 25	
196	3 12	0 22	
210	3 36	0 14	
224	4 12	0 36	
238	5 00	0 48	
245	5 30	0 30	
252	6 30	1 00	
256	7 30	1 00	
144·7	Mean		

Weight.	Curvature.	Difference.	No. 17.
14 lb	0° 08′	0° 00′	Broke. Splinter 4 in.
28	0 30	0 22	
42	0 50	0 20	
56	1 09	0 19	
70	1 30	0 21	
84	1 50	0 20	
98	2 12	0 22	
112	2 38	0 26	
126	3 00	0 22	
140	3 42	0 42	
154	4 30	0 48	
168	5 30	1 00	
182	6 30	1 00	
98	Mean		

Weight.	Curvature.	Difference.	No. 18.
14 lb	0° 06′	0° 00′	Broke. Splinter 12 in.
28	0 16	0 10	
42	0 26	0 10	
56	0 36	0 10	
70	0 48	0 12	
84	0 56	0 08	
98	1 12	0 16	
112	1 26	0 14	
126	1 36	0 10	
140	1 48	0 12	
154	2 00	0 12	
168	2 15	0 15	
182	2 40	0 25	
196	3 00	0 20	
210	3 24	0 24	
224	3 48	0 24	
238	4 12	0 24	
252	4 54	0 42	
266	6 00	0 06	
270	7 00	0 00	
277	8 30	0 30	
152·7	Mean		

Weight.	Curvature.	Difference.	No. 19.
14 lb	0° 06′	0° 00′	Broke. Splinter 18 in.
28	0 12	0 06	
42	0 18	0 06	
56	0 24	0 06	
70	0 30	0 06	
84	0 45	0 15	
98	0 54	0 09	
112	1 06	0 12	
126	1 14	0 08	
140	1 24	0 10	
154	1 36	0 12	
168	1 48	0 12	
182	2 00	0 12	
196	2 12	0 12	
210	2 24	0 12	
224	2 36	0 12	
238	2 54	0 18	
252	3 12	0 18	
266	3 30	0 18	
280	3 54	0 24	
294	4 30	0 36	
308	5 30	0 00	
161	Mean		

Weight.	Curvature.	Difference.	No. 20.
14 lb	0° 06′	0° 00′	Broke. Splinter 2 feet.
28	0 18	0 12	
42	0 28	0 10	
56	0 36	0 12	
70	0 44	0 08	
84	0 54	0 10	
98	1 02	0 08	
112	1 12	0 10	
126	1 20	0 08	
140	1 30	0 10	
154	1 40	0 10	
168	1 50	0 10	
182	2 02	0 12	
196	2 14	0 12	
210	2 26	0 12	
224	2 44	0 18	
238	3 06	0 22	
252	3 30	0 24	
266	4 12	0 42	
280	5 00	0 48	
294	6 00	1 00	
301	7 30	1 30	
160·7	Mean		

Weight.	Curvature.	Difference.	No. 21.
14 lb	0° 06′	0° 00′	Broke. Splinter 2 feet 6 in. long.
28	0 16	0 10	
42	0 26	0 10	
56	0 36	0 10	
70	0 48	0 12	
84	0 56	0 08	
98	1 08	0 12	
112	1 18	0 10	
126	1 30	0 12	
140	1 42	0 12	
154	1 54	0 12	
168	2 12	0 18	
182	2 22	0 10	
196	2 32	0 10	
210	2 50	0 18	
224	3 12	0 22	
238	3 30	0 18	
252	3 54	0 24	
266	4 30	0 36	
280	5 18	0 48	
294	6 30	1 12	
301	7 30	1 00	
160·7	Mean		

Weight.	Curvature.	Difference.	No. 22.
14 lb	0° 06′	0° 00′	Broke. Splinter 18 in. long.
28	0 14	0 08	
42	0 24	0 10	
56	0 36	0 12	
70	0 48	0 12	
84	1 00	0 12	
98	1 12	0 12	
112	1 24	0 12	
126	1 36	0 12	
140	1 50	0 14	
154	2 06	0 16	
168	2 24	0 18	
182	2 48	0 24	
196	3 06	0 18	
210	3 42	0 36	
224	5 00	0 18	
119·2	Mean		

Weight.	Curvature.	Difference.	No. 23.
14 lb	0° 06′	0° 10′	Broke short at fulcrum.
28	0 16	0 10	
42	0 26	0 10	
56	0 36	0 12	
70	0 48	0 08	
84	0 56	0 16	
98	1 12	0 08	
112	1 20	0 10	
126	1 30	0 12	
140	1 42	0 14	
154	1 56	0 16	
168	2 12	0 12	
182	2 24	0 12	
196	2 44	0 20	
210	3 06	0 22	
224	3 30	0 24	
238	3 54	0 25	
252	5 30	0 54	
133	Mean		

Weight.	Curvature.	Difference.	No. 24.
14 lb	0° 06′	0° 00′	Broke short at the fulcrum.
28	0 14	0 08	
42	0 22	0 08	
56	0 30	0 08	
70	0 38	0 08	
84	0 46	0 08	
98	0 54	0 08	
112	1 00	0 06	
126	1 10	0 10	
140	1 20	0 10	
154	1 30	0 10	
168	1 40	0 10	
182	1 50	0 10	
196	2 00	0 10	
210	2 12	0 12	
224	2 24	0 12	
238	2 42	0 18	
252	2 50	0 08	
266	3 06	0 16	
280	3 30	0 24	
294	3 48	0 18	
308	4 12	0 24	
322	4 54	0 42	
336	7 00	0 06	
175	Mean		

Weight.	Curvature.	Difference.	No. 25.
14 lb	0° 06′	0° 00′	Broke short at fulcrum.
28	0 14	0 08	
42	0 22	0 08	
56	0 30	0 08	
70	0 38	0 08	
84	0 46	0 08	
98	0 54	0 08	
112	1 02	0 08	
126	1 12	0 10	
140	1 22	0 10	
154	1 30	0 08	
168	1 40	0 10	
182	1 50	0 10	
196	2 00	0 10	
210	2 12	0 12	
224	2 24	0 12	
238	2 36	0 12	
252	2 54	0 18	
266	3 08	0 14	
280	3 30	0 22	
294	3 48	0 18	
308	4 30	0 42	
322	5 30	1 00	
329	6 00	0 30	
336	7 00	1 00	
181·2 Mean			

Section of the piece of pitch pine out of which the 25 pieces were cut, and the mean weight each piece bore.

1 153·7	2 145·8	3 147	4 139·5	5 151·9	6 152·6
12 152·6	11 152·6	10 132·6	9 104·9	8 145·9	7 160
13 161	14 153·7	15 144·7	16 98	17 152·7	18 161·0
19 160·7	20 160·7	21 119·2	22 133	23 175	24 181·2

Average weight each piece would sustain; 148·44 lb.

Mean of the greatest weight each piece bore; 274·3 lb.

The average time that each piece was under trial was 8′ 42″.

Description of the Apparatus for trying Experiments on the Strength of Timber. (Plate LXV.)

The pieces of wood which were the subjects of these experiments were fixed so as to be firmly held by one extremity, whilst a load was applied at the other, sufficient in the first instance to bend it; and the weight was gradually increased till the piece was broken; the degrees of curvature which the different accessions of weight produced were noted by means of a divided arch attached to the extremity of the piece.

To fasten the piece firmly in its position, a beam of oak, A A, (Fig. 1) one foot square was fixed vertically, and firmly secured between the floor and the ceiling. In the middle of this, at B, was a mortice, six inches by two inches, to receive the end of the piece of wood, B D, which was firmly secured by driving in two wedges, *a*, *b*, from opposite sides, so as to hold it fast down upon the lower side of the mortice. The piece, B D, was planed true to its intended dimensions; and for the purpose of applying the load, as well as to ascertain the degree of flexure, an arch, E, was fastened on at the extremity, by a tenant at the end of the piece being inserted, and keyed into a mortice made through the middle of the arch; and to keep it firm, a short piece of rope, *e*, was made fast to the upper end of the arch, and clenched to the middle of the piece, as shown in the figure.

The weights were placed in a scale, F, suspended by a double rope, which was applied upon the circumference of the arch, as shown in the edge view of it at *f f*, and made fast to it at the top. By this means, as the rope, *f f*, always drew at a tangent to the arch, E, the effective leverage of the load, F, to bend or break the piece was in all cases very nearly in direct proportion to the weight applied in the scale; and in order to obtain the neat weight independent of the weight of the scale and the arch, a small line, *g*, was fastened at the lower end of the arch, and carried between the double ropes, *f f*, as shown in the edge view. This line being conducted over two pulleys, G, G, had a weight, H, applied, sufficient to counterbalance the weight of the scale and the arch. The arch, E, was divided into degrees and parts, which were numbered from the middle upwards. The angle of curvature was pointed out by the edge of an index, *k*, made of a piece of plate fastened by a screw against a prop, K. This index was capable of a slight adjustment before the experiment began, in order to bring it to zero. Whilst the piece of wood continued without any load, and therefore without any flexure, all weight applied in the scale after this having a direct tendency to bend the piece, the degree of flexure produced by any given weight was noted by the degrees and parts as shown by the index upon the divided arch. It would sometimes happen when the piece was cross-grained, and much loaded and bent down, that it would get a tendency to twist, and the arch would go over sideways. To prevent this, a line was made fast on each side to the upper end of the arch, and during the experiment two persons held the ends of these lines, and kept the arch upright by pulling, so as to counteract any tendency it might have to go sideways, though without increasing or diminishing the load which tended to bend or break the piece.

Article IV.

On the Power that Spiders have of conveying their Threads from one Point to another, and of flying through the Air. By Carolan.

(To Dr. Thomson.)

SIR,

As the following experiments tend to elucidate the method by which the geometrical spider conveys itself and its threads from one place to another, and exhibits some curious facts connected with that phenomenon, I thought that an account of them might not be uninteresting to some of your readers, especially as the subject has never been thoroughly investigated.

In order to ascertain the nature of the transitive power which the spiders of this class possess, I filled a plate with water; and having put a piece of pipe-clay between two and three inches in diameter into the middle of it, I stuck a straw about a foot long into the clay, so as to stand perpendicularly, and placed two small dry stones on each side of the straw to cover the clay, lest the operations of the spider should be impeded by its moisture. I then put a geometrical spider on the straw, and set the plate on a table at some distance from any object. The spider ran up and down the stones and the straw the whole day, without making its escape. The water was particularly offensive to it when it touched it, as it always ran back immediately whenever it came into contact with it. I left it on the table all night, and in the morning it had made its escape. I observed a line drawn from the top of the straw to the roof of the room, and fixed there, at a distance of about four or five feet. The thread was almost straight upwards. I could not conceive how it accomplished this, without supposing that it had either flown in some way, or had shot out its thread to that length before it went off.

I got another spider of the same kind; and having put it on the straw, it endeavoured to make a passage from its confinement much more readily than the first. Having dropped down by its line about an inch from the top of the straw, it seemed to fix its thread round its middle legs, and resting itself while hanging in this way against the straw with its head and fore legs. Its hindmost legs being stretched out behind it, in a few moments it shot out a thread from its spinners about a yard long. The thread went straight out, rising gradually upwards. It continued floating in this way for a minute or two, when the spider turned round, took hold of it with its fore legs, and began to pull it in. The thread was flying so much upwards as to form a very acute angle with the short line upon which the spider rested; but when the spider drew it in, it became more horizontal. It appeared to guide its line as a boy does a kite. While it drew in the threads very quickly with its fore legs, what was taken in was formed into a round ball upon its hindmost legs, which was left upon the straw. It seemed in calm air to have the power of making the line move slowly round it, like a long feeler. When the thread was blown upon, so as to change its position, and throw it into waves, it quickly returned to its former place and elongated form. It at last caught hold of the arm of a chair within its reach; and as the thread continued hanging loose, the spider pulled it in, till it became tight; and when it found it sufficiently fastened to bear it, it ran along it, strengthening it by another thread much thicker as it moved on. The spider being very young, it was very difficult to observe the thread, on account of its great tenuity; but I was much assisted in perceiving it by the particles of dust which stuck to it. I tried the same experiment with several other geometric spiders, and observed nearly the same results; but as they are mostly very young at present, it was not easy to discover their operations correctly, from the extreme exiguity of the threads.

Having at length, however, procured one much larger than those I had formerly examined, I placed it on the post of trial, as usual, in such a situation that the sun shone full upon it, which enabled me to observe its movements precisely. The geometrician soon emitted, with surprising quickness, a pretty long line; and as I wished to examine the end of it, to see if there was any thing peculiar in its conformation which caused it to stick so readily to any thing it touched, I broke the thread close by the straw, and drew it in by degrees. The only thing I could observe was, that it became more and more tenuous, till it turned almost invisible. This form of the line is perhaps necessary to its rising in the air. The spider then shot out another thread, which it strengthened by emitting a second alongside of the first, but not quite so long. After the two threads had united into one, it attached the line to the straw; and after drawing it in again with its fore legs till it became very short, and finding it did not catch hold of any thing, not having been sufficiently long to reach the surrounding objects, it abandoned it, and remained at rest for some time, as if preparing for a greater effort to make its escape. It then dropped down by its thread an inch or two from the top of the straw, and ejected from its spinners two threads at once, a good deal longer than the former ones, and added immediately a number of lines in the same direction extremely exile; but as the reflection of the sun's rays was very bright from them, I could count about 14 distinct threads, which issued from the small apertures of which the spinners are composed. They soon all joined in one; and as the spider seemed still to lengthen them, and guide them as if by magic, it evidently emitted from its spinners a stream of air, or it may be possible some subtle fluid of the electric kind, for the purpose of stretching out and coalescing the different filaments of which the thread was formed, as there was something which ran along the whole thread bringing it more into a horizontal position, making a kind of obtuse angle as it went along with the part that was flying upward. It then turned round as usual; and fixing the line to the one it was hanging by, seemed to guide its movements for a few minutes, when the line fastened to the wall. It then drew it tight, and went across. I then put the spider upon the straw, and took it out to the garden. The wind was blowing very strong, and it rather seemed averse to move; but as I kept it in motion by touching it gently, it made another attempt to escape, as it found its situation not quite agreeable. From the time it continued letting out its thread, it must have been many yards long, as the wind seemed greatly to facilitate the emission. The thread was, however, thrown to the ground and broken by an unsettled gust of the wind.

They send out their threads with such celerity that they could eject, I should think, about 30 yards in a minute. It is surprising that in a room, where the air is quite still, they should be able to make their threads fly straight out or upwards with such rapidity. We would be apt to imagine that in a substance so very light, the part which was sent out last would move quicker than the part which was before it, and consequently get into curves or knots. It makes it, therefore, more probable, that these spiders must have the power of throwing out some stream of air, or some subtle fluid, as the line keeps moving as straight out as a fishing-rod, as long as the spider pleases, and never is inclined to fall down, but always rises.* While one was guiding its thread which it had sent out to some length, being suspended as usual by a short line from the top of the straw, I observed that when I blew upon the thread that was flying, it raised the spider up a little. By catching hold of the flying thread with my finger, I tried to draw the spider upwards; and I drew it several feet from the place where it hung, it having let out a line behind it to that length. I conceived that in this way, with the help of other circumstances, they might be carried through the air, by a thread of some length, to a great distance.

Nor was this conjecture wrong; for having kept a geometrical spider running for some time upon my hand, which was stretched out a little, it dropped down about six inches from the point of my finger by its thread, and immediately emitted a pretty long line at a right angle with the one by which it was sespended. The thread which was flying outwards quickly rose upwards, and carried the spider along with it. When the spider had ascended as far above my finger as it was before beneath it, it let out the thread which was attached to my finger, and continued flying smoothly upwards till it nearly reached the roof of the room, when it veered about to the side, and alighted on the wall. When it flew its motion was smoother and quicker than when a spider runs along the thread. If they are able to fly so easily in a room, they will evidently fly with much more facility in the open air. These spiders generally drop down from the place on which they rest some inches by their thread before they shoot out their flying line; that by hanging in the air they may be enabled to feel more sensitively whether the line they have let out may be buoyant enough to carry them up, or whether it fixes on any object while they pull it in. They may fly in this way to any length, or to any height; for as the line lengthens behind them, the tendency to rise increases.

I found also that another species of spider, with a bright yellow body, and very short legs, had the power of shooting out threads, but not to the same extent as the geometric spider.

I tried several other kinds of spiders, but none of them seemed to have the power of making their escape, remaining eight or ten days on the straw. They were as lively after being without food during that time as when first brought from the fields. Two could never live together on the same place. The stronger very soon killed the weaker one: and they always seemed to be conscious of their respective degrees of strength. The more feeble was always more afraid of its enemy than any thing that could be presented to it. When touched gently with the finger now and then, they did not regard it much, and sometimes hardly moved out of their place; but when they saw a spider of superior strength coming near them, they sometimes ran into the water to avoid him. One having fallen accidentally into the water, upon which it floated without being able to get out, I was going to extricate it, when it sank to the bottom, and ran along the surface of the plate, on which there was some gravel, beneath the water, with the same rapidity as if on dry ground, like a small crab. When it came to the side it could not emerge, and appeared unwilling to try it, as the water made its legs clap so close together when brought out of the water, that it could not move. I observed, when it was first immersed in the water, it discharged two large air bells from its sides; from which I still think that it is very probable they may render themselves lighter by internal air, although it might possibly be the air of respiration, as most insects breathe laterally. These trials prove beyond a doubt that the geometric spider has the power of shooting out threads to an indefinite length, and of flying by means of the thread; and I think this curious circumstance was only once observed before by some person in France. The manner, however, in which they fly was never before noticed, nor attempted to be accounted for, with any feasibility. How admirably is every creature fitted for the sphere of its existence! and how much does it add to our pleasure, that, while we survey the varieties in nature, we ever behold the wisdom of our Creator!

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* If the torpedo has the power of throwing out electricity, may not the spider have the same power to a certain extent, though exercised in a different way?

ARTICLE V.

On the Cells and Combs of Bees and Wasps. By Mr. Barchard.

(To Dr. Thomson.)

MY DEAR SIR, *Oct.* 2, 1816.

In a paper which I had the honour of sending you some time since, I promised you some observations which I had made with regard to the cells and combs of bees, wasps, &c. in consequence of Dr. Barclay's ideas that the cells were separate, or composed of two walls, &c. Should you consider the following worthy of insertion, I shall feel myself honoured, and remain,

Sir, yours respectfully,

R. W. BARCHARD.

Bees' Cells and Combs.

Bees' cells, as is generally known, are composed of regular hexagons, terminated at bottom by three rhombuses, so disposed that the bottom of one cell comes exactly on the wall or division of the opposite; thus giving them the greatest possible strength: the cells lying horizontal, one above the other; thus forming a vertical comb from the top of the hive to the bottom. Dr. Barclay having been able to divide the partitions of the cells into two leaves or separate cells, supposed them to be separately composed and stuck together. I have been for some length of time in the habit of keeping bees, to which I have paid great attention; and, in the first place, consider it very much against the general economy of the bee to suppose they should bestow the time and pains in making separate cells, *i. e.* double partitions, when single ones would suffice, particularly as bees appear to enjoy every thing in common. If a piece of virgin comb is examined, by cutting it asunder at right angles to the cell, it will appear a homogeneous mass regularly shaped, but without any appearance of division. It will also appear, on examining the comb, that some of the cells are much higher than the others; that is, one high cell and one low one, frequently joining. Now if the cells were double, we should see the part of the high one that is above the other thinner than the bottom; but this is not the case: thus tending to prove that they are only single. But if we take a piece of old comb that has had wood in it, and cut it in the same way, there is every appearance of its being double. In fact, we may frequently divide it into several leaves; but we must not consider this the original structure; for the young bee or maggot, in going through its chrysalis state, spins itself a fine web, which lines the entire inside of the cell. After the young one leaves it, the old ones, or workers, instead of clearing it out, stick it up tight to the sides; thus giving it the appearance of double partitions, but which in fact is nothing more than a number of skins sticking to the original structure.

Wasps' Combs.

The cells and combs of wasps are *vice versâ* of bees'; that is, vertical cells and horizontal combs, with the mouth or opening of the cell upwards. The combs' stratum or superstratum supported on pillars, with just sufficient space between for the wasps to move. The composition is decaying wood gathered from palings, generally oak, which the animal grates off with a strong pair of nippers, with which it is furnished, and then sticks together with an animal glue into a papyrus-like substance. The wood does not undergo any alteration; for with a common eye-glass the pieces may be seen in their natural state, evidently showing that the cells are single; for the pieces may be distinguished on both sides of the same cell. Now in the papyrus-like substance that envelopes the whole nest, it is evidently composed of several layers.

Hornets' Combs, &c.

Are likewise composed of rotten or touch wood in a hollow tree, which they clear out for the purpose. They are *vice versâ* of wasps' combs. Thus are the cells open, or have the mouth downwards. These cells are evidently single; for the pieces of which they are composed are sufficiently large to be distinguished by the naked eye; and the same piece may be distinguished on both sides of the same cell, the wood also being in different states of decay, the layers evidently showing that they are begun from the top, and carried downwards.

* * *

As you sometimes give place in your *Annals* to some particular cases in surgery, at least as far as connected with natural history, I beg to inclose to you the following case of hydrocephalus or hydatids on the brain of a sheep:—

About the middle of January, 1815, a young sheep was observed by the shepherd to be unwell, from its heavy and stupid appearance, such as standing still and bleating, and continually losing itself, or leaving the flock to which it belonged; the disease gradually, but continually, increasing, until it put on the certain symptoms of water gathering on the brain; that is, by the animal constantly inclining to one side, until at last it describes a circle, which it gradually decreases until it is unable to move more than the length of its body. From the first appearance of the disease until its fatal termination is generally from two to three months. The present subject was taken from a large flock, and put into an enclosure close by my house; thus giving me an opportunity of seeing all its actions. One day, being frightened, it ran into a deep pond, in which it continued swimming in a rotatory manner until nearly exhausted before it could be got out. I am at present unable to determine as to the cause of the animal always turning to the affected side, whether it proceeds from a partial paralysis of the side affected (as they do not seem to feel the effect of the nerves crossing) or from the animal constantly leaning its head to the painful side. The hydatid seldom occupies more than one lobe of the brain, sometimes posterior, sometimes anterior, is situated beneath the dura mater, and depresses the substance of the brain; but there does not appear to be any part of it absorbed. That part of the cranium immediately over the hydatid is so much reduced by absorption, that it is found to yield by pressure of the thumb and finger. Indeed, I have seen it completely absorbed, and the brain protruding itself between the cranium and scalp, the animal still living in that state. This case was operated upon about six weeks from its first appearance, by turning back the scalp (having first ascertained the situation of the hydatid by pressure) and applying a triphine immediately, the bone was removed. The cyst protruded through the orifice, and was taken out by the fingers. A slight hemorrhage took place, which stopped by gentle pressure. The wound was then dressed, and healed in about a month. The animal was afterwards turned into some grass pasture, and appears going on well. Simple puncture does not answer; for unless the cyst is removed, the water accumulates again.

P. S. Your correspondent S. appears to have mistaken my ideas with regard to the thermometer scale. If he re-peruses that article, he will find that I said Fahrenheit's scale was arbitrary, and recommended the decimal scale beginning with zero.

ARTICLE VI.

A Description of a new portable Barometer and a new Hygrometer.

By Daniel Wilson, Esq.

THE high degree of perfection which physical science has attained is in a great measure to be ascribed to the excellence of our philosophical instruments. Until experimental investigation was assisted by the thermometer, barometer, and air-pump, few or no important natural facts were ascertained, or general laws of matter established; whilst every subsequent discovery of an additional agent has opened a new field of scientific inquiry.

Within these few years, since the study of geology has become more general, and that the observations of men of science have been directed to the investigation of the influence of elevation on vegetation and meteorology, much attention has been bestowed on rendering the barometer portable, and on simplifying its application to the mensuration of heights. The labours of Ramsden, Deluc, General Roy, Sir George Shuckburgh, Dr. Hamilton, Dr. Maskelyne, Professor Leslie, Professor Playfair, Dr. Macculloch, M. Fortin, and M. Biot, have been at various times successfully engaged in promoting these objects; and more particularly by Sir Henry Englefield and M. Gay-Lussac, the common barometer has been brought to as great a degree of perfection with regard to portability as its principle seems to admit of. But from the length of the mercurial column required to balance the weight of the atmosphere, it must always be an inconvenient instrument to the traveller; especially since it is oftenest used in ascending those more inaccessible regions where a trifling weight becomes a burden of magnitude.

It is perhaps for this reason that barometrical observations, in their application to the admeasurement of heights, have not yet become so general as their importance merits.

By constructing a new barometer on just principles, possessing at the same time accuracy and portability, I cannot but hope that some facilities will be given to the prosecution of this department of science, and that in a short time it will be enabled to keep pace with the general progress of discovery.

The principle upon which this instrument depends is the regular expansion or contraction of a permanently elastic fluid on a diminution or increase of the pressure under which it exists.

The objection to an instrument of this kind, and it appears at first to be insurmountable, is the difficulty of measuring the expansion of the included air, which must vary according to its quantity; but this objection is entirely overcome by having the elastic fluid so placed that, in proportion as the weight of the atmosphere decreases, it continues to throw upon itself a column of mercury until its height balances the diminished pressure without. The quantity of expansion is not, therefore, the measure required; it is the height of the mercurial column, as in the common barometer, without reference to its diameter.

This instrument is composed of a cistern of iron turned truly cylindrical, and is one inch in its internal diameter, and 1$\frac{3}{4}$ inch in depth. The top and bottom of this cistern are also made of iron, and adapted to it by screws.

Through the centre of the top a glass tube about 10 inches long is made to project into the cistern exactly to half its depth, and is there firmly cemented. The bore of this tube is about $\frac{1}{15}$ of an inch in diameter, and must be chosen as perfectly cylindrical as possible.

At the side of this tube a delicate thermometer is likewise introduced through the top of the cistern, so that its bulb shall be a short distance within it. It must also be cemented in, as well as the top and bottom of the cistern, so that the whole shall be perfectly air tight.

The cistern is now filled with mercury to such a height that the end of the tube inserted in it is immersed under a greater quantity of that fluid than the whole length of its bore could contain, which prevents it from ever being uncovered in any possible position of the instrument so that the portion of included air could escape. This is most conveniently done by drilling a small hole through the side of the cistern at the calculated height, and pouring in mercury by the glass tube until it flows out of this aperture, which is then secured by a screw and cement. The temperature of the cistern is brought to 75° Fahr. at the time of sealing. A small quantity of mercury is afterwards added until it stands in the tube at a convenient height above the cistern.

We have now a close vessel containing a portion of air confined by mercury, and exposed to the variations of weight in the atmosphere. The obvious consequence of any diminution of its pressure will be a proportional expansion of the included air, and an elevation of mercury in the tube, until its re-action forms a counterpoise to the expansive force of the confined elastic fluid.

The height of this column will be in proportion to the diminished pressure without; but an inch of the common barometer will be expressed by less than an inch on this instrument, from the mercury sinking in the cistern as it rises in the tube, and because a certain force is absorbed in the increased space which the included air must occupy from the rising of the mercury. The height of the mercurial column will, therefore, be a mean proportional between the action and re-action of these different forces, and it is a fixed quantity subject to no disturbing cause except temperature.

In order to graduate this instrument, the end of its tube is cemented into a glass vessel, connected with an air-pump and a condensing apparatus, to which an accurate barometer gauge is attached. The new instrument is placed in an upright position, with its cistern resting in a vessel of water at the temperature of 75° Fahr. The air in the receiver is now condensed until the standard barometer indicates 32 inches, when the height of the mercury in the new instrument is to be accurately marked. The receiver is then exhausted until the barometer falls to 20 inches, and the position of the mercury is again carefully ascertained in the new instrument. It will be found that, as the exhaustion goes on, for every inch the mercury falls in the standard barometer, it will rise in the proportion of about ·75 of an inch in the new instrument, and the same ratio will be preserved throughout all the space.

The range thus obtained is divided into twelve equal parts, which represent barometrical inches.

The temperature of the new barometer must be preserved the same during the operation, which the included thermometer indicates with much precision. It is also necessary to ascertain the temperature of the barometer gauge; and if it varies from 60° Fahr. or any other fixed point which it may be found convenient to assume, the well-known correction for diminution or increase of gravity in the mercury must be applied to the new barometer, by raising or lowering the fixed points, in order to form a proper compensation. As the temperature of the external air acts alike on both instruments, it is not necessary to observe it. By this means the new barometer at the temperature of 75° Fahr. will always correspond in its indications with the common barometer when the mercurial column of the latter is at 60° Fahr., whatever may be the heat of the atmosphere. But a variation in the temperature of either instrument will occasion a disagreement in their indications; on this account a correction for heat is necessary.

This correction is very simple, from the circumstance of gases undergoing precisely equal augmentations of bulk from equal increments of temperature, and as mercury is governed at a low heat by the same law. Its amount is ascertained by immersing the cistern in water of different temperatures until the rate of expansion is obtained. It must also be observed that no change takes place during the operation in the state of the barometer. The value of a given number of thermometrical degrees in thousandths of an inch is engraved on the barometer scale, and the manner of applying it as a correction will be afterwards explained. But entirely to prevent the necessity of any correction, it is found convenient to bring the instrument, during each observation, to the temperature at which it was graduated. This is readily effected by holding it in the hand, or under the arm, for a short time before it is to be used; and the cooling of the cistern is so slow, from its being incased with tubes preserving a small space between each other, as to allow of sufficient time for making the observation without risk of error.

The temperature of 75° Fahr. is chosen, from its being one which can easily be obtained by the heat of the body in almost every climate.

In order to prevent the escape of mercury when the instrument is inverted, a small piece of porous wood is inserted in the extremity of the bore of the tube, which allows of the free passage of air, but prevents the transmission of a denser fluid. The tube is afterwards terminated by a stop-cock cemented on so as to be air-tight.

The instrument thus prepared is inclosed in a brass tube having a slit in front about three quarters of an inch wide, which reaches from the top of the cistern to the extremity of the scale. This front slit shows the fixed thermometer and barometer tube and scale, which is divided in the usual manner, and may be read off to the thousandth of an inch by means of a vernier moveable by a screw. Above the fixed thermometer a detachable one is placed for taking the temperature of the air. A narrow slit behind gives light to enable the observer to make a tangent of the lower edges of the vernier with the convex surface of the mercury, as in the best kind of barometer. Another thin brass tube, slit in the same way, is afterwards fixed on, which, by turning half round, covers the front aperture in the usual manner; and the top is terminated by a small ferrule, or cap, which unscrews, and shows a milled head for moving the vernier, and a steel ring by which the instrument may be suspended.

The barometer is now completed. In order to employ it in the mensuration of heights, the temperature of the air is taken in the usual manner, by means of the detached thermometer. During the time occupied in doing this, the cistern of the barometer is held in the hand, under the arm, or other warm part of the body, until the temperature is raised a few degrees above 75° Fahr., the point at which it is graduated. This will be accurately ascertained by the included thermometer. When that degree of heat is indicated, the stop-cock is opened, and the barometer suspended by the iron ring, or rested on the knee in the usual manner. A few gentle taps on the tube will make the mercury oscillate freely, and take its proper level. As the temperature approaches the standard, the vernier is moved by the milled head, so that its lower part shall be a tangent to the convex surface of the mercury, when that degree is indicated by the fixed thermometer. The change of temperature, as before observed, is sufficiently slow to admit of perfect accuracy. The height being then read off and registered, the observation is completed; and the same means may be employed for deducing its metrical value as with the common barometer.*

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* Mr. Thomas Jones, of Charing Cross, has just published a set of very useful tables on this subject. He has undergone the labour of calculating by the loga-

In climates where the temperature of the air is above 75° Fahr. opposite means must of course be employed to cool the cistern, and the observation be made on its rising to the proper degree. In such a case the correction will be more simple. At all events, the observer has it in his power to choose either means.

Before the brass tube is turned back to cover the slit, the barometer is held in a slanting position with its cistern uppermost, so that the mercury may descend slowly in the tube, and force the air before it through the porous piece of wood. When all the air is expelled, and the mercury rests on the wood, the stop-cock is shut, in order to prevent the entrance of air, which might tend to separate the mercury in the tube, although it could not enter the cistern. By this means the instrument is rendered perfectly portable, and not liable to be put out of order, however much it may be agitated. When carried, the cistern should be kept uppermost. The size of the instrument is only about 12 inches long, and 1¼ inch in diameter, so that it can readily be put into the pocket.

When it is not found convenient to bring the barometer to the temperature already specified before each observation, the degree of the fixed thermometer must also be read off at the different stations, and registered, and the value in parts of an inch of its deficiency of 75° added to, or its surplus subtracted from, the barometrical quantity, according as it is above or below the standard temperature.

It has already been noticed that the linear value of a specified number of thermometrical degrees is always engraved on the scale of each instrument, in order to facilitate this correction.

When the instrument is made as a domestic or marine barometer, it is fitted with a sliding scale, on which the thermometrical degrees are engraved. By inspecting the thermometer, the temperature is to be ascertained, and the sliding scale is moved until the number on it which corresponds to the deficiency of temperature in the barometer from the proper standard is on a line with the mercurial column. The top of the sliding scale is then pointing to the true barometrical height. This correction is simply adding the quantity which the mercurial column would acquire in height if raised to the standard temperature. By making this standard above the general heat of the atmosphere, the correction is always additive, and therefore easily performed, and not likely to create any confusion.

The advantages which this instrument possesses over the common barometer in the mensuration of heights will be immediately perceived. Its greater portability would be sufficient to entitle it to a preference, but it likewise promises to be more accurate in practice. It is well known that a considerable difference exists in the specific gravity of the mercury which is met with for sale; and the makers of barometers have not the means, or could not bestow time, for purifying all that they use. It is, therefore, employed in the state

rithmic tables the metrical value from 15 inches to 31 inches of the barometer in thousands of an inch; so that the approximate height in feet is at once obtained by inspection, from the difference of the observations. The corrections for temperature are likewise easily performed.

which they buy it; and the barometer is graduated without reference to the gravity of the mercury which it contains. A constant source of error arises from this circumstance, which will be avoided in the instrument that I have described; as it will be only necessary to ascertain with accuracy the specific gravity of the mercury with which the standard barometer is filled. Both extremes of the new instrument being derived from it, a compensation will be effected for any difference in the gravity of the mercury which is employed.

In the common barometer another source of error arises from the difficulty of ascertaining the temperature of its column of mercury, which may be considerably heated by holding it in the hand without perceptibly effecting the thermometer placed in the mounting. In fact, this correction is too often omitted entirely, from the uncertain way in which it can be obtained. From the thermometer being included within the cistern of the new instrument, the real temperature will always be indicated; and as it is graduated at a fixed point, and a correction made for any variation of temperature in the standard barometer, this source of inaccuracy will be entirely removed.

The principle upon which this instrument is constructed may be yet further extended; and, by substituting lighter fluids for the mercury, barometers of such delicacy may be formed as to be practically employed with advantage in levelling. And if it be found, on experience, that the confined atmospherical air loses a portion of oxygen by acting on the mercury, hydrogen or nitrogen may be employed, which will undergo no change.

In atmospherical phenomena, the instrument next in importance to the thermometer and barometer is, perhaps, the one which ascertains variations in its degree of moisture. Within these few years much progress has been made in this department of meteorology, which, in its relation to the comforts and welfare of mankind, is confessedly of the highest importance. Many eminent men have directed their labours to its advancement, and its instruments have consequently been multiplied and improved. But one which is with regard to moisture what the thermometer is to heat, still remains a desideratum in the science.

A great variety of hygrometers have been at different times constructed by Smeaton, Saussure, Deluc, and other scientific men; and more lately Professor Leslie and Captain Kater have invented very excellent instruments of this kind, which have given much insight into the phenomena of the weather. It is well known that Professor Leslie has for some years devoted himself to this study with the most eminent success.

The principles upon which hygrometers have been constructed are, the absorption of heat by evaporation, the changes which take place in the length of animal and vegetable fibres by alterations in their degree of moisture, the variations in the capacity of a hollow ball of ivory from the same cause, and likewise the changes in weight of deliquescent bodies.

The objections to all hygrometers that have yet been invented are their tendency to change in sensibility, the necessity of some manipulation, or their not being comparative.

To overcome all these objections would be no easy matter. I will not venture to assert that I have done so; but the instrument to be described is more simple and sensible than any I have yet seen.

During a variety of experiments made some years ago in the pursuit of hygrometry, I found that slips of animal bladders were very susceptible of changes in humidity. The idea naturally suggested itself of trying whether the internal capacity of the whole bladder did not likewise change with the degree of moisture to which it was exposed. I put this to the test of experiment with the gall bladder of a sheep filled with mercury, and I obtained an unlooked-for delicacy and precision in the result. I found that, on its being repeatedly immersed in water of the same temperature, the mercury always fell to the same point, and that it likewise always rose to the same height when included in air exposed to the absorbent power of sulphuric acid of the same specific gravity. This range is considerable, being at least three times as much as would be caused in a thermometer between the freezing and boiling points, in which the bulb and tube were in the same ratio to each other as the bladder and tube of the hygrometer.

This seems a general property of bladders. I have tried those of many animals, but give the preference to the urinary bladder of a rat,* on account of its small size, and its extreme sensibility.

In order to construct this instrument, procure the urinary bladder of a rat, or other small animal, wash it well in cold water, and turn it inside out. By means of a file, notch a small part of the extremity of a thermometer tube, and insert it a little way into the orifice of the bladder; then tie it on firmly with a silk thread, and make a puncture with a fine needle immediately below the fastening, in order to allow the air to escape as the mercury enters. The most convenient method of filling it is to put rather more mercury than what is necessary into the gall bladder of a lamb; this bladder, previously moistened, is then attached in a temporary way to the other extremity of the thermometer tube, and by gently inclining it the mercury will run down, and displace the air in the rat's bladder, and ultimately escape through the puncture. When this takes place, the hole must be secured, by extending the tying of the silk thread over it, and making it fast. It is necessary to keep the rat's bladder wet during the operation, and not to allow too great a column of mercury to press on it, otherwise it will be apt to burst when in this state.

The mercury is now to be adjusted, until it stands at a convenient height in the tube, when the bladder is immersed in water at the temperature of 60° Fahr., which is the point of extreme moisture. Extreme dryness is obtained by inclosing the bulb in a glass vessel containing a portion of sulphuric acid of the specific gravity 1·850. This range is divided into 100 equal parts, if the bore of the tube be equal, commencing at extreme moisture; and is so graduated on the scale to which the instrument is attached.

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* It may perhaps lead to some useful result to state that I have found the rats in London to be very subject to urinary calculi; which I did not find to be the case in other towns. The bladders of some are entirely filled with a white spongy matter; others with red gritty sand. I have not yet submitted any of these calculi to analysis.

Dr. Thomson has suggested in his *Annals* that it would be better to reverse the scale by placing 0 at the point of extreme dryness, and 100° at that of extreme moisture. Whatever comes from so eminent a chemist is entitled to the fullest consideration. My principal reason for making zero at extreme moisture is, that it is a more invariable point than extreme dryness, and therefore, perhaps, a more certain foundation for calculation. The point of extreme dryness will vary with the specific gravity of the sulphuric acid which is used, as well as with temperature; but extreme moisture, by depending on water alone, is subject to no variation on that account, and the temperature is in this operation more readily maintained at the desired point. I acknowledge that, on the first view of the case, it appears proper that the numbers on the scale should decrease with a corresponding decrease of moisture; but it is also to be considered that the dryness produced by sulphuric acid is by no means the absence of all moisture, but merely a relative term. With more powerful absorbents mercury may be raised higher; on the contrary, immersion in water is absolute dampness, and no means which I have discovered can make the mercury sink lower.

The graduation of the instrument having in this manner been completed, a porous piece of wood is inserted in the end of the tube, and it is terminated by a small brass cap. The correction for temperature is ascertained either by obtaining the ratio between the capacity of the bladder and the bore of the tube, which, together with the rate of expansion of mercury, will give the length of a thermometrical degree; or, by immersing the instrument in water at different temperatures, and marking the rise of the mercury. This correction will vary in every hygrometer, for the same reason that causes a difference in the length of thermometrical scales.

The hygrometer in this state, with its bulb uncovered, answers very well as a domestic instrument; but when it is agitated the column of mercury is apt to burst the bladder. In order to render it portable the bulb is inserted through the top of a cistern of box-wood incased with brass. This cistern has its bottom composed of a leather bag, which is acted on by a screw, and is likewise incased by a brass tube.

When the instrument is to be packed for carriage, the cistern is rather more than half filled with mercury, and screwed to its cap, through which the bladder is inserted. By screwing up the leather bottom of the cistern, the air within it will be displaced through the pores of the box-wood, and the mercury will wholly encompass the bladder, and gradually force the mercury of the hygrometer to rise in the tube. When it has risen to within half an inch of the top, the screwing is to be discontinued.

It is evident that by this contrivance the pressure is wholly thrown off the bladder, and that no degree of agitation can ever affect it. Another advantage is, that it gives a power of insulating the instrument, and of only bringing it into action when necessary; by which means it will be preserved uninjured for any number of years even in water.

I do not find that, in the ordinary state of the atmosphere, the bladders are subject to any alteration in their properties. I have had some past me as hygrometers for above three years, and there is no perceptible change in their delicacy. But it is not proper that they should be allowed to remain for a length of time in water, as, like every other animal matter in such a situation, they are liable to decay.

I do not consider it necessary here to enlarge on the uses and applications of the hygrometer, as it is presumed that the greater number of persons interested in science are already fully acquainted with them. A permanent system of meteorology can only be founded on the multiplied observations of individuals on different parts of the earth. Theories which are built on a less solid foundation are soon overturned and forgotten; but every insulated fact will be carefully preserved as an additional step towards the formation of that perfect system which will at last, from the accumulation of data, truly explain the nature of all atmospherical phenomena.

In a future paper I will take an opportunity of detailing the result of experiments on the power of various absorbents, and other circumstances connected with hygrometry.

These instruments have been constructed for me by Mr. Thomas Jones, of Charing Cross, who was originally employed by Sir H. Englefield to make his mountain barometer, and who still continues to make them. Mr. Jones has deservedly acquired much reputation for the accuracy of his works; and I am highly indebted to his skill and ingenuity in overcoming many practical difficulties in the formation of this barometer and hygrometer, and in bringing them to their present degree of perfection.

Note.—I may here remark, for correction, that in a former paper (vol. viii. p. 125) the word *solubility* has been printed for *volatility*, which alters the sense. It has escaped being noticed in the errata.

Fig. 1.

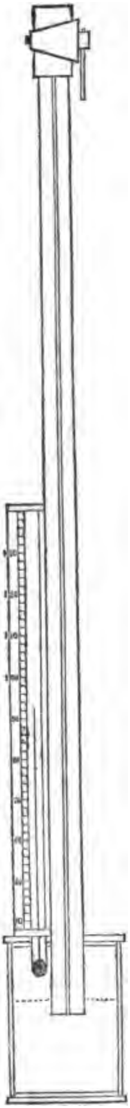


Fig. 1 is a section of the barometer cistern, showing the manner in which the tube and thermometer are placed.

The dotted line marks the height of the mercury, the space above being filled with air.

Fig. 2.

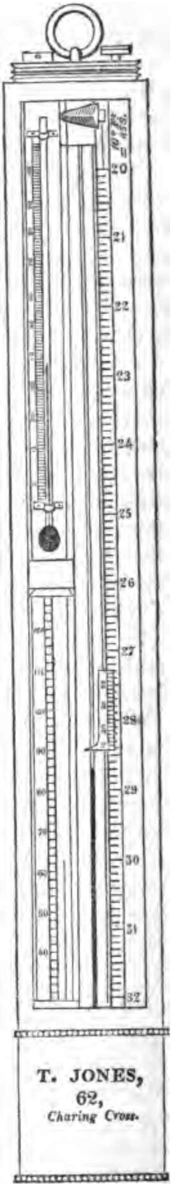


Fig. 2 is the barometer in its finished state, in which the different parts are represented as they appear when the sliding tube is turned round and the cap at top unscrewed.

ARTICLE VII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, Feb. 27, a paper by Sir Everard Home, Bart. was read, giving an account of a number of fossil bones of the rhinoceros found in a lime-stone cavern near Plymouth by Mr. Whitby. Sir Joseph Bankes had requested Mr. Whitby, when he went to superintend the *breakwater* at present constructing at Plymouth, to inspect all the caverns that should be met with in the lime-stone rocks during the quarrying, and to send him up any fossil bones that might be found. The fossil bones described in this paper occurred in a cavern in a lime-stone rock on the south side of the Catwater. This lime-stone is decidedly transition. The cavern was found after they had quarried 160 feet into the solid rock. It was 45 feet long, and filled with clay, and had no communication whatever with the external surface. The bones were remarkably perfect specimens. They were all decidedly bones of the rhinoceros; but they belonged to three different animals. They consisted of teeth, bones of the spine, of the scapula, of the fore legs, and of the metatarsal bones of the hind legs. They were compared by Sir Everard with the bones of the skeleton of a rhinoceros in the possession of Mr. Brookes, which is considered as belonging to the largest of the species ever seen in England. The fossil bones were mostly of a larger size, though some of them belonged to a smaller animal. Several of them were analysed by Mr. Brande. He found one specimen composed as follows:—

Phosphate of lime	60
Carbonate of lime	28
Animal matter	2
Water	10
	100

The teeth as usual contained a greater proportion of phosphate of lime than the other bones. These bones were remarkably clean and perfect, and constitute the finest specimens of fossil bones ever found in this country.

At the same meeting, two papers by Thomas Knight, Esq. were announced as presented to the Society: a paper on the Construction of Logarithms, and a paper on the Functions of Differences.

On Thursday, March 6, a paper by the Rev. Francis Hyde Wollaston was read, describing a thermometer constructed by him for determining the height of mountains instead of the barometer. It is well known that the temperature at which water boils diminishes as the height of the place increases at which the experiment is made, and this diminution was suggested, first by Fahrenheit, and afterwards by Mr. Cavendish, as a means of determining the height of places above the sea. Mr. Wollaston's thermometer is as sensible as the common mountain barometer. Every degree of Fahrenheit on it occupies an inch in length. The thermometer, together with the lamp and vessel for boiling water, when packed into a case, weighs about a pound and a quarter, and is much more portable and convenient than the common mountain barometer. It is sufficiently sensible to point out the difference in height between the floor and the top of a common table. Mr. Wollaston gave two trials with it, compared with the same heights measured by General Roy by the barometer. The difference between the two results did not exceed two feet.

On Thursday, March 13, an appendix to Mr. Pond's paper On the Parallax of the Fixed Stars was read. Conjecturing that the small difference which occasioned the suspicion of a parallax was owing to the difference between the heights of the external and internal thermometers in summer and winter, Mr. Pond endeavoured to keep the inside of the observatory last winter of the same temperature as the outside, which the mildness of the season enabled him to accomplish. Many observations on α Lyræ were made. No deviation whatever was observed; or, if any minute deviations existed, they were in an opposite direction from that of a parallax.

At the same meeting, part of a paper by Mr. Marshall on the *Laurus Cinnamomum*, or cinnamon-tree, was read.

On Thursday, March 20, Mr. Marshall's paper on the Laurus Cinnamomum was continued. He took a review of the descriptions of this plant given by preceding botanical writers, and pointed out numerous mistakes into which they had all fallen, from not being aware of the meaning of the different names given to the plant and its varieties by the natives of Ceylon. Linnæus gave to his laurus cassia the properties of the laurus cinnamomum; and Thunberg, the last botanist who describes this tree, does not correct the errors of his predecessors, and probably was not aware of their existence. The cinnamon-tree is cultivated in four different places in Ceylon, and it grows wild abundantly in the jungles. The cinnamon obtained from the cultivated places amounts to rather more than 2000 bales, and that collected in the jungles is about an equal quantity. What is called *cassia* is the receptacle and unripe seeds of the laurus cinnamomum.

LINNÆAN SOCIETY.

On Tuesday, March 4, a paper was read, communicated by Dr. Leach, from the manuscripts of the late Col. Montague, describing a new genus of vermes distinguished by the name of *amphiro*. Five British species were described. They are all inhabitants of the sea, distinguished by long tentaculæ, organs of respiration, and substances which answer the purposes of feet.

At the same meeting, a paper by T. A. Knight, Esq. was read, containing a vindication of his hypothesis respecting the cause why the radicles of plants vegetate downwards, and the stems upwards, against the attack made upon it by the Rev. Patrick Keith in the last volume of the Transactions of the Linnæan Society. Mr. Knight admits that, if his hypothesis had been supported only in the way in which it has been represented by Mr. Keith, the refutation of it would have been very easy: but Mr. Keith, he affirms, has omitted the principal arguments which he had advanced in support of it. This he admits was owing to a defect of memory on the part of Mr. Keith. But he conceives that every person who takes upon himself to controvert the statements of another ought in honour to be in a state to represent these statements fairly, and that he is responsible for the accuracy of the representation which he gives of the opinion of another.

Mr. Knight then proceeded to give his arguments in favour of the hypothesis which he advanced, and showed the omissions of which Mr. Keith had been guilty. He next adverted to the facts which Mr. Keith has brought forward in opposition to Mr. Knight's hypothesis, and gave an explanation of them. He concluded his paper by some observations on Mr. Keith's own hypothesis, *instinct*, which he considered as unsatisfactory and unmeaning.

On Tuesday, March 18, a paper by Sir James Edward Smith, Pr. L. S. was read, elucidating some obscurities in the genus *Tordilium*. The author shows that the species apulum and officinale have been frequently confounded by preceding botanists. He points out the distinction, and explains the proper references.

At the same meeting was read a description, by Dr. Leach, of the Wapiti deer, a species of animal from the banks of the Missouri, four of which, brought from America by Mr. Taylor, are at present exhibiting in the King's Mews, London. The animal is gentle, docile, and elegant. It is said to be domesticated in America by the natives. Mr. Taylor is of opinion that it might be used with advantage in this country in many cases as a substitute for horses.

At the same meeting a letter from Sir John Jamieson to Mr. Macleay was read, giving an account of a striking peculiarity in the ornithorinchus paradoxus of New Holland. Sir John Jamieson, who is at present in New Holland, shot one of these animals with small shot, and his overseer went and picked up the wounded animal. It ran one of its spurs into his hand. In a short time his arm swelled, his jaw became clenched, and he exhibited all the symptoms of persons bitten by venomous serpents. The symptoms yielded to the external application of oil and the internal of ammonia; but the man suffered acute pain, and had not recovered the use of his arm in a month. On examining the spur, it was found to be hollow; and on pressing it a quantity of venom was squirted out. For what purpose the animal is supplied with this venom does not appear, though probably it is to wound and destroy its prey.

ARTICLE VIII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Dr. Merriman and Dr. Ley will recommence their lectures on Midwifery, and the Diseases of Women and Children, on Monday, April 21, at the Middlesex Hospital.

II. *New Method of taking the Specific Gravity of Gases.*

(To Dr. Thomson.)

SIR,

HAVING often been disappointed in procuring that degree of exhaustion in a globe which is necessary for the weighing of different gases, a point of no inconsiderable moment with the practical chemist, in considering the subject, the following appeared to me an easy and expeditious method of forming (in my opinion) a most perfect vacuum, without that liability of failure which is attendant upon the use of the air-pump, where you can seldom procure a sufficient degree of exhaustion for so delicate an experiment.

The apparatus consists of a glass globe, *a* (Plate LXVI., Fig. 3), mounted with an iron stop-cock, *b*, which by means of a screw is able to be fixed to the iron tube, *c*, also furnished with a stop-cock, *d*, at its upper end, which tube should be 32 inches long; *e* is a small glass funnel.

Having placed the apparatus in the position, fig. 3, with the glass globe screwed to the tube, open the stop-cocks, and by means of the glass funnel, *e*, fill the globe and tube with quicksilver, remove the funnel, and turn the cocks. It may now be turned with the globe uppermost (Fig. 4); and if the mouth, *d*, be introduced under the surface of quicksilver in a proper vessel, and the stop-cocks opened, the quicksilver will descend, leaving the globe and part of the tube in a state of most perfect exhaustion; by fastening the cock, *b*, it may be removed from the tube for use.

Should the above possess sufficient novelty and utility to merit a place in your *Annals*, you will oblige me by its insertion.

I am, Sir, yours truly,

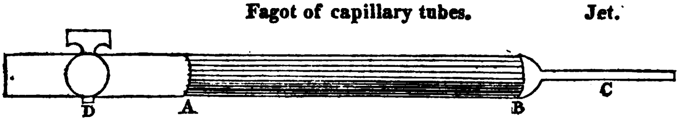
Barnet, Feb. 9, 1817. THOMAS S. BOOTH.

III. *Further Improvement in Brooke's Blow-pipe.* By Dr. Clarke.

(To Dr. Thomson.)

SIR,

Dr. Wollaston having suggested the expediency of increasing the number of the tubes, rather than the diameter, for the jet of the gaseous blow-pipe, I have, in consequence, adopted a plan for the passage of the gas, where a great degree of heat is requisite, that will not expose the operator to any chance of explosion; using, at the same time, Professor Cumming's safety cylinder containing oil. It is this: let A B represent a fagot of capillary brass tubes, with the smallest possible diameters, all communicating with one tube at C, whose diameter, at the least, should equal $\frac{1}{45}$ of an inch, and the whole being on the outside of the stop-cock, D, of the jet, it will be evident that, if the gas in the tube C be exploded, there will only be a partial detonation, extending its effects only as far as B.



EDWARD DANIEL CLARKE.

IV. *Canvas Tubes for conveying Water.*

(To Dr. Thomson.)

SIR, *Glasgow, Jan.* 1817.

Some time ago one of your correspondents gave much credit to the French nation for superior ingenuity, in exemplification of which he gives them the sole merit of inventing and using canvas tubes for conveying water. That these tubes may have been used by the French, cannot be denied; though I must be allowed to say that they are seldom either deficient, or very just, in making such claims; but at what period these tubes were used in France, your Correspondent has not stated. About 1800 or 1801, I proposed to Mr. John Wood, ship-builder, Port Glasgow, the application of these tubes as the best mode of obviating the difficulties experienced by seamen in supplying themselves with water on foreign, difficult coasts. Mr. Wood said that he had for many years used them for filling from the adjacent harbour holds of vessels under repair. As these canvas tubes appear to be but partially known, permit me shortly to state the plan. Let vessels be provided with pipes of necessary length, and of about two inches diameter, made of canvas, without any paint, and with a small portable tin or copper pump, 2½ or 3 inches in diameter, and about eight or nine inches in length of stroke. By this simple apparatus water may be easily and expeditiously conveyed to their boats without landing casks, &c.; also casks in the vessel's hold may be filled from the boat without altering the stowage; thereby saving much tear and wear, besides much dangerous, and at times unsuccessful, labour. From the lightness, cheapness, facility of construction, and durability, of these tubes, may they not be successfully applied to the ventilation of coal-mines, as well as to various other useful purposes? After such celebrated men have been labouring towards improvement in the ventilation of mines, it is with great diffidence that I venture to lay before you the outlines of a plan that has for some time occupied my thoughts. Let series of tubes (Pl. LXV. Fig. 5), each extending from one headway to another, and with one end shaped like a funnel, so as to receive the small end of the following tube, and at the same time admit a current of air from without, be fixed along the roofs of the different lateral workings, and all run into a main tube that shall extend from the lowest dip to the upcast pit, and be there connected with a cylinder or pump wrought by some of the working gears. Here let one or more gasometers, similar to those for the gas-lights, be constructed, having a safety valve, connected with an upright or vertical tube, running up the shaft into open day. From these gasometers let a small tube be carried along one of the safe headways used by the miners down along the bottom, or its angle, where there is the best security from accidents. From this tube let small lateral tubes, either fixed or flexible, each furnished with a stop-cock, run off opposite to each place of work. Jets, with cocks, may also be fixed wherever they may be required. By now setting the pump a working the gasometers will fill with the carbureted hydrogen collected from the workings through which the lateral tubes extend, and then the gas acting on the safety valve will make its escape up the upright pipe to the surface, until the mine becomes clear from the gas, when there could be no risk in allowing flame to be applied to each of the jets and tubes, for the purpose of supplying the miners with abundance of cheerful light in their dreary abodes. Such, Sir, is an attempt to verify the proverb of turning our greatest bane to our greatest benefit, by converting the scenes of catastrophe, so prevalent of late, into scenes of illumination. Persons better acquainted with mines, and their localities, will no doubt be able to suggest many improvements. It may be objected that, though the pipes may succeed in clearing the workings from accumulated carbureted hydrogen, still danger may occur from blowers, or unforeseen vacuities, or wastes in the strata. To prevent this I would propose that a safety lantern be screwed upon each jet: and if any lantern proposed be thought too expensive, I will in my next propose a simpler and much cheaper plan of safety lantern.

I remain, Sir, most respectfully,
Your most obedient servant,
Hugh Wallace.

V. *On Hannibal's softening the Rocks of the Alps by Vinegar.*
By Col. Beaufoy.

Historians say that Hannibal by fire and vinegar softened the Alpine rocks, and made a passage for his army into Italy. May not this apparently strange assertion be thus explained :—

By means of wood to make fires he supplied his pioneers who were cutting the road through the rocks with water, by melting the ice and snow, to satisfy the excessive thirst produced by the copious perspiration in those elevated regions where the air is so attenuated and dry, and he caused vinegar to be mixed with the water more effectually to satisfy the thirst.

VI. *Chemical Nomenclature.*

(To Dr. Thomson.)

SIR,

Chemical students are much embarrassed by the abundance of synonyma. It is, on that account, very desirable that there should be a uniformity at least in authors of the same period; but this is far from being practised. I am of opinion that this diversity of chemical names is to be ascribed chiefly to the want of distinct rules for their formation, agreeably to the established analogies of our language. Why, may I ask, are not yttria and barytes spelled, as recommended by Young—ittria and barites? Some authors write barita, silica, alumina, &c. Now for what reason are these preferable to silex, alumine, &c.? I know no one so capable of answering and deciding these points as the Editor of the *Annals.* He will much oblige a constant reader by his reply; and more particularly so, if he will have the goodness to translate the following French names into the language of the two theories according to which they are given:—1. *Acide muriatique oxigéné.* (Why is not oxide spelled with *y* as well as oxygen?) 2. *Acide muriatique suroxigéné.* 3. *Acide muriatique hyper-oxigéné*; or *chlore, acide chloreux,* or *oxide de chlore,* and *acide chlorique.*

Z.

I am afraid it would be difficult by any rules to secure a regular chemical nomenclature; because the contrivers of the rules would have no means of enforcing the observance of them. Perhaps there is no great injury sustained by chemistry by the great number of new names that have been of late proposed. They give beginners, in the mean time, a little trouble; but the bad ones gradually fall into disuse, while the good ones only are ultimately retained.—When foreign words are introduced into the English language, it has been generally the practice to retain the mode of spelling employed in the language from which the word is borrowed. This is my reason for writing *yttria* and *barytes* instead of *ittria* and *barites,* which would have the disadvantage of keeping out of view the etymology, without being more conformable to the rules of our language than the original words.—Oxide is a contraction of the two Greek words οξυς and ειδος. It might be written oxeide, but it could scarcely, with propriety, be converted into oxyde, as some, however, have done. We know three compounds of chlorine and oxygen. Chlorine is the oxymuriatic acid, or acid muriatique oxygéné, of former chemists. The compounds of chlorine and oxygen I call—

1. Protoxide of chlorine; the euchlorine of Davy.

2. Deutoxide of chlorine; the more recently discovered gas of Davy, to which he gave no name.

3. Chloric acid; the hyper-oxymuriatic acid of former chemists.

I am not aware of the terms acide muriatique sur-oxygéné, or chlorous oxide, having been employed by chemists. A discussion on nomenclature would occupy too much room for this place; but I would recommend Mr. Chenevix's very judicious essay to the attention of my correspondent. In the new edition of my System of Chemistry I shall be at some pains to give all the synonymes, while I adopt the terms that strike me as most systematic.—T.

VII. *On the Mode of detecting Lime in Sugar of Lead.*

(To Dr. Thomson.)

SIR,

As it is an object of some importance to the consumers of sugar of lead to be enabled to ascertain the state of its purity; and as the sugar of lead of commerce is frequently known to be adulterated with acetate of lime; I should be glad to know by what means this adulteration can be detected.

Dr. Henry, in the last edition of his Chemistry,* directs us to add to a dilute solution (of the acetate of lead) oxalic acid; but does not this acid precipitate both the lime and oxide of lead, even when the solution is extremely diluted? $\frac{10}{24}$ gr. of sugar of lead dissolved in 1 oz. of distilled water has, in some experiments made by me, afforded a precipitate which, if I am not very much deceived, was all oxalate of lead.

I am, Sir, your obedient servant,
A Constant Reader.

* * *

The most accurate way of analyzing sugar of lead mixed with acetate of lime is the following:—Dissolve 100 grains of it in pure water which has been recently boiled: or, if common water be used, redissolve the precipitate that forms during the solution, by adding a drop or two of nitric acid. Through this solution pass a current of sulphureted hydrogen gas, obtained by dissolving sulphuret of iron in sulphuric acid, or sulphuret of antimony in muriatic acid, till the whole of the lead is precipitated in the state of a sulphuret. Then filter the solution, and pour into it carbonate of potash or soda. If a precipitate fall, it may be presumed to be carbonate of lime. This precipitate must be washed and dried. Every 100 parts of it indicate the presence of 156·8 grains of acetate of lime in the sugar of lead.

There is another method of separating lead from lime in solution in the same acid, which is still easier of execution, and which, therefore, I may mention here:—Tartaric acid precipitates lead, but it does not throw down lime immediately, provided the solution be not too much concentrated. Into a diluted solution of sugar of lead (completely dissolved by means of nitric acid) drop tartaric acid as long as any precipitate falls. Then filter the solution, and set it aside. In 24 hours the tartrate of lime (if any be present) will be deposited in minute crystals.—Nitrate of lime enables us to separate oxalic acid from tartaric. Dissolve the mixed acid in water, and pour nitrate of lime into the dilute solution. The oxalate of lime falls; but the tartrate remains in solution.—T.

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* Seventh edition, vol. ii. p. 472.

VIII. *Query respecting Stains of the weaker Acids.*

(To Dr. Thomson.)

SIR, *Bedford-square, March* 10, 1817.

I shall feel myself much obliged by you, or any of your correspondents, favouring me, through the medium of the *Annals*, with a ready method of destroying the stains made by the less caustic acids, as those of citric, oxalic, &c. It has been sometimes asserted that an alkali will destroy them; but I have ineffectually tried that method; and, I imagine, should any method be discovered, it would be found extremely useful.

With regard to your information respecting diabetic urine, I feel myself extremely obliged to you as far as it concerns the Annales de Chimie; but not having devoted much time to the study of foreign languages, your information would be more acceptable in the English, Latin, or French, rather than in the Swedish, German, or other languages.

I am, Sir, with the most profound respect,
Your obliged humble servant,
T. L.

IX. *Query respecting Artificial Camphor.*

(To Dr. Thomson.)

DEAR SIR,

If you will have the kindness, by means of your *Annals*, to state the process of preparing artificial camphor, you will much oblige,
Yours truly,

March 16, 1817. J. MORSON.

For a description and history of the mode of making artificial camphor, I refer my correspondent to my System of Chemistry, fourth edition, vol. v. p. 74.—T.

X. *Chemical Composition of Minerals.*

I have considered the ingenious letters of An Electro-Chemical Theorist with some attention, and agree with him that his mode of calculation is necessary for bringing the analysis of minerals under the pale of the atomic theory. The theory of Berzelius is different, and indeed is nothing else than the reduction of all analyses under his grand chemical canon, that the *oxygen in acids is always a* *multiple by a whole number of the oxygen in the bases.* This is the bed of Procrustes, on which he places every chemical analysis; and by the application of the requisite degree of *stretching* or *lopping off* he brings every one to the proper dimensions. As neither the nitrates nor the phosphates can be reduced under this canon, it is obvious that it cannot with propriety be made the foundation of a chemical theory so important as the constitution of the mineral kingdom.

I have lately made some modifications in the atomic theory, which have removed most of the anomalies under which it formerly laboured. These will appear when the new edition of my System of Chemistry, at present in the press, is published. But I may state here, for the satisfaction of my correspondent, the mode of calculation which I am in the habit of following. I consider the weight of an atom of the different ingredients in the mineral kingdom to be as follows:—

Silica	2·000
Alumina	2·125
Magnesia	2·500
Glucina	3·250
Lime	3·625
Soda	4·000
Protoxide of nickel	4·375
Protoxide of iron	4·500
Protoxide of manganese	4·500
Phosphoric acid	4·500
Yttria	5·000
Potash	6·000
Strontian	6·500
Protoxide of cerium	6·750
Barytes	9·75 &c.

Procure a logarithmic scale similar to Dr. Wollaston's scale of equivalents, and write the names of the different mineral substances opposite to the numbers, denoting the weight of an atom of each. When the analysis of a mineral is to be examined, bring the quantity of one of the ingredients, as the silica, upon the slide opposite to the weight of an atom of silica on the rule, then the numbers opposite to the quantities of the other ingredients on the slide will stand opposite to the weights of the atoms of each or of some multiple of that weight, supposing the mineral to be a chemical compound, and to be accurately analyzed. Thus the number of atoms of each constituent is easily obtained; after which our chemical knowledge of the way in which the constituents combine in other cases will enable us to state the constitution of the mineral with sufficient accuracy.

With respect to the chemical analysis of a mineral, it is not sufficient that the analyst be a correct chemist; he must likewise be a skilful mineralogist; or the specimen examined must be put into his hand by a skilful mineralogist, and carefully selected for the purpose, otherwise little confidence can be put in the result, because the mineral examined may be either wrong named or impure. Hence it is important that every analysis should be preceded by a mineralogical description of the specimen subjected to analysis. Were it not that it would have an invidious appearance, I could easily exhibit specimens of very faulty analyses by first-rate chemists from inattention to the purity of their specimens.

XI. *On Bees' Combs.*

(To Dr. Thomson.)

DEAR SIR,

When writing to you a short time since on bees' combs, &c. I forgot to mention the manner in which I succeeded in parting the combs into the separate cell-like appearance. If a piece of virgin comb be put into water just below boiling, it will be seen gradually to melt down and spread itself on the surface of the water, without leaving any insoluble residue; but if a piece of old comb that appears black, and has had brood in it, be put into water, the cells will gradually crack, and may then be easily separated into distinct pieces; the theory of which is this, not that the cells were, or even are, separate; but, as I said before, after the young brood has left the cell, the skin with which it was enveloped, and which lines the entire inside, instead of being taken out by the old bees, is stuck up to the sides. Therefore, when the comb is put into hot water, the wax of which the cells were first composed is melted, and the skinny linings are separated into distinct pieces, which to a casual observer would appear as though the cells were absolutely made distinct, but which is not the case, as we see by the virgin comb dissolving without any residue or skin-like substance.

I remain, Sir, yours respectfully,

Waddon, Nov. 19, 1816. R. W. BARCHARD.

XII. *Introduction of Vaccine Lymph into America.*

Render to Cæsar the things that are Cæsar's.

(To Dr. Thomson.)

SIR,

With much surprise I have seen it stated in Mr. Pettigrew's Memoirs of the Life and Writings of the late Dr. Lettsom that "the vaccine lymph was first sent by him across the Atlantic, and consigned to the fostering care of Dr. Waterhouse, Professor of the Theory and Practice of Medicine in the University of Cambridge, Massachusetts; and that from him it was spread through the United States." But this is not true. Vaccine lymph had been previously sent by Dr. George Pearson to Dr. John Chichester, now a resident physician at Bath, but at that time in very extensive practice at Charleston, in South Carolina. An opportunity soon offered for the employment of it, of which Dr. C. availed himself; and the particulars may be seen by a reference to Mr. Tilloch's

Phil. Mag. vol. xvi. p. 252, where all the particulars are stated by Dr. Rhodes, who witnessed the insertion of the matter, and the progress of the vaccine disease throughout its whole course. The subject was afterwards tested with variolous matter, and the constitution found proof against its effects.

Your friend and constant reader,

London, March 13, 1817. G.

Article IX.

New Patents.

John Hague, of Great Pearl-street, Spitalfields; for improvements in the method of expelling the molasses or syrup from sugars. July 27, 1816.

Robert Salmon, surveyor, Woburn, Bedfordshire; for further improvements in the construction of machines for making hay. July 27, 1816.

John Poole, of Sheffield, victualler; for brass and copper plating, or plating iron or steel with brass or copper, both plain and ornamental, and working the same into plates, bars, or other articles. Aug. 3, 1816.

John Chalklen, of Tower-street, Seven Dials, London; for improvements in or on valve water-closets. Aug. 3, 1816.

William Henry, of Manchester, Doctor of Physic; for improvements in the manufacture of sulphate of magnesia, commonly called Epsom Salts. Aug. 3, 1816.

John Dayman, of Tiverton, gentleman; for a method of covering or coating iron, steel, and other metals, or mixtures of metals with tin, lead, copper, brass, or other metals, or mixtures of metals. Aug. 3, 1816.

Article X.

Scientific Books in hand, or in the Press.

Mr. Brown, of St. Germains, Cornwall, is preparing for the press an agricultural work on the irrigation of land, which he will treat of in a perfectly novel manner.

Sir William Adams is about to publish A Practical Inquiry into the Causes of the frequent Failure of the Operations of extracting and depressing the Cataract, and the Description of a new and improved Series of Operations, by the Practice of which most of these Causes of Failure may be avoided.

Mr. Mill's long expected History of British India is now in the press, and will be published in three 4to. volumes.

Dr. Spurzheim's new work, entitled, Observations on the deranged Manifestations of the Mind, or Insanity, is in the press.

Mr. James Sowerby is preparing a Midland Flora, comprising the indigenous plants of the more central counties.

ARTICLE XI.

METEOROLOGICAL TABLE.

1817.	Wind.	BAROMETER.			THERMOMETER.			Hygr. at 9 a. m.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
2d Mo.										
Feb. 8	N W	30·27	30·21	30·240	50	43	46·5	60	1	☾
9	N W	30·29	30·10	30·195	51	39	45·0	61		
10	Var.	30·10	29·80	29·950	50	33	41·5	77	—	
11	Var.	30·00	29·38	29·690	41	28	34·5	63	·34	
12	N W	29·77	29·38	29·575	46	33	39·5	88	7	
13	S W	29·45	29·40	29·425	53	38	45·5	60		
14	S W	29·75	29·43	29·590	53	35	44·0	83	·42	
15	N W	29·66	29·43	29·545	50	35	42·5	53	3	
16	S W	29·90	29·66	29·780	47	32	39·5	79	—	●
17	S W	30·02	29·90	29·960	55	45	50·0	73	—	
18	N W	30·09	30·02	30·055	54	34	44·0		3	
19	S W	30·15	29·75	29·950	46	38	42·0	63	—	
20	S W	29·50	29·38	29·440	48	33	40·5	63	6	
21	S W	29·58	29·36	29·470	44	34	39·0	60	—	
22	N W	29·90	29·58	29·740	47	33	40·0	65		
23	W	29·79	29·68	29·735	49	39	44·5	53	4	
24	N W	29·95	29·79	29·870	48	38	43·0	63		☽
25	W	29·69	29·62	29·655	51	40	45·5	62	·11	
26	W	29·81	29·54	29·675	47	40	43·5	70	·10	
27	N W	29·79	29·54	29·665	50	39	44·5	59	3	
28	S W	29·76	29·67	29·715	54	43	48·5	62	—	
3d Mo.										
March 1	W	29·68	29·47	29·575	53	32	42·5	58		
2	S W	29·38	29·18	29·280	49	35	42·0	60	·14	
3	S W	29·14	28·84	28·990	50	36	43·0	59	·50	○
4	N W	29·24	29·14	29·190	45	30	37·5	60	—	
5	W	29·24	28·78	29·010	47	34	40·5	65	·25	
6	W	29·22	28·78	29·000	43	28	35·5	62		
7	W	29·10	28·98	29·040	46	34	40·0	63	—	
8	N W	29·40	29·10	29·125	43	28	40·5	60	·55	
9	N W	29·91	29·40	29·655	45	29	37·0	61	—	
		30·29	28·78	29·592	55	28	42·06	64	2·68	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Second Month.—8. The light of an Aurora Borealis was very perceptible about ten, p. m. through the clouds which overspread the sky to the N: a windy night, with a little rain, followed. 9. Calm: grey sky, with the lighter modifications: at sun-set the clouds exhibited a splendid set of tints: close to the horizon was a clear space, lemon-coloured; above this, crimson lights, with shadows of grey and purple, in a variety of figures, streaked, waved, and clustered; of those in the E some were rose-red, others a tender green: a windy night ensued. 10. Fair: roads dusty. 11. Snow, a.m. with a gale at NE: in the night a southerly gale, with rain. 12. Showers. 13. Misty: small rain: windy. 14. a. m. *Cirrostratus:* gloomy: fair day, with clouds: windy night, with rain. 15. Windy night. 16. Windy: *Cumulus* beneath linear *Cirrus*, passing to *Cirrostratus*. 17. Cloudy: some rain morning and evening. 18. Dripping at intervals: windy. 19. a.m. Calm: the dew drops frozen clear on the grass: a very fine day ensued, with *Cumuli*, and a breeze: windy night. 20. Much wind at S this evening. 21. Fleecy *Cumuli* beneath a hazy sky, with the lighter modifications: inosculation and *Nimbi* followed, with rain, sleet, and snow. 22. Fair: sun and clouds. 23. Windy: shower at night. 24. A bright haze at sun-rise and sun-set. 25. Fine day: some *Cirrostrati* assumed an arrangement not very frequent, of discs piled obliquely on each other. 26. *Cumulus*, capped with *Cirrostratus:* lunar corona. 27. After a gale through the night, rain before nine, a.m.: *Nimbi*, with hail, p.m.: at night large *Cirri*, very conspicuous by moonlight, stretching SE and NW. 28. Fair, save a light shower.

Third Month.—1. Fair: windy. 2. A trace of solar halo about nine, in some *Cirri*, which soon subsiding went off with the wind to SE, grouping into forms like the crown of the *Nimbus: Cumulostrati* succeeded, which, p.m. gave place again to *Cirrose* obscuration, with a southerly gale and showers at night. 3. a.m. Overcast: p.m. steady rain: at sun-set a hazy sky, and much vapour: a highly rarefied *Cumulostratus* in the SE: a hard gale, with rain, at night. 4. Pale sky, a.m.: after which passing *Nimbi* and a little hail: calm night. 5. Hoar frost: fair, with *Cumulus* and *Cirrus:* evening, very large *Nimbi:* shooting stars: wind. 6. Wet morning: then fair, with various clouds: night frosty. 7. Pretty thick ice: fair day: rain at night. 8. Windy: snow in flakes about 1½ in. diam.: sleet and rain: at noon large *Cumuli* in the N, passing to *Cumulostrati*, the sky above them being blue to 15° of the cyanometer: about two, p.m., a sudden shower of hail from a dense lofty *Nimbus:* the balls were opaque, in the form of a cone with a rounded base about ½ in. diam., and composed entirely of striæ meeting at the apex of the cone: we have had similar hail repeatedly of late: frost (after rain) at night. 9. The lighter modifications prevailed, a. m. the *Cirri* pointing to NW: after these lofty *Nimbi* formed in the midst of groups of *Cumulus*, letting fall slight showers: the night was clear frost.

RESULTS.

Winds Westerly.

Barometer:	Greatest height	30·29 inches.
	Least	28·78
	Mean of the period	29·592
Thermometer:	Greatest height	55°
	Least	28
	Mean of the period	42·06

Mean of the Hygrometer, 64°: its drier extreme several times about 40°.

Rain..2·68 inches.

The Aurora Borealis, which has appeared several times in the period, has not been well seen, for want of a clear sky, in this neighbourhood. On the 2d and 3d of the present month there were violent thunder-storms to the W and S, the latter of which came as near to us as Tunbridge, but neither of them was much perceived here, save in the evident electric state of the clouds on the latter evening.

Tottenham, *Third Month*, 24, 1817. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

MAY, 1817.

Article I.

Observations on the Flame of a Candle. By Mr. Porrett.

(To Dr. Thomson.)

DEAR SIR,

MUCH valuable information has recently been given to the public relating to flame. Sir H. Davy, in his Notice of Experiments, and new views respecting it, dated July 21 last, and published in the third number of the Journal of Science and the Arts, has stated, that when a flame emits a brilliant light, it is always owing to the production and ignition of solid matter, and that in the particular instance of the combustion of a stream of coal gas in the atmosphere, the solid matter produced is charcoal, originating in "the decomposition of a part of the gas towards the interior of the flame where the air is in smallest quantity, and which, first by its ignition, and afterwards by its combustion, increases in a high degree the intensity of the light." By intercepting the flame of a stream of burning coal gas with a piece of wire-gauze, and observing in what situations it became blackened, this distinguished chemist has shown that neither the summit of the flame, nor the lower part of it, where the gas burns blue, are the portions in which the charcoal is separated; but that in the intervening parts it is given off in considerable quantities.

The next important paper on flame is that by George Oswald Sym, M. A. which appeared in the number of the *Annals of Philosophy* for November last. This gentleman, by means of some ingenious dissections of the flame of a candle with a piece of wire-gauze, has shown that the flame is merely superficial, forming an elliptical bubble filled with volatile matter not arrived at the inflamed surface; and he has asserted that, were the flame solid, the wick could not be seen through it; for flame, he adds, "is an opake substance, as any one may satisfy himself by trying to read a book through the upper part of the flame of a candle."

Having paid some attention to the appearance of the flame of a candle, with the advantage of a previous perusal of the papers just mentioned, it appears to me that my observations are more precise than those already made, on one or two points, and impart some little additional information. I consider it, therefore, right to make them public, because every improvement, however slight, in our knowledge of this subject, derives some importance from the connexion which necessarily exists between such knowledge and the means resorted to for increasing the beneficial effects of flame, or for preventing its destructive agency.

The first observation which I made on looking at the flame of a candle was, that the luminous portion of it is surrounded on all sides by a flame nearly invisible. The reason why it is not easily seen is, that the eye is not in a condition to observe the feeble light emitted by this exterior flame while it is under the influence of the brilliant light emitted from the surface of the interior flame; but if this last-mentioned light is diminished by any means, then the exterior flame becomes more apparent. Thus it is better seen when a candle burns with a dim light for want of snuffing; still better when the flame of a candle is in extensive contact with a metallic surface, by which its light is materially diminished; and best of all in those flames which give very little light, as in that of spirits of wine. The weakly luminous exterior flame is the part really undergoing the process of combustion and producing heat; and there seems reasons for believing that no part of the atmospheric oxygen penetrates beyond it, and that the heat of other parts is merely acquired by contact with it.

I find that, when a piece of wire-gauze of about 900 meshes to the square inch is cut as nearly as possible into the size and shape of the flame of a candle, or rather of that part of it which rises above a short wick, and the central wire is left to descend about three-fourths of an inch below the rest, to represent a wick, so that this wire may be thrust down the middle of the true wick of a candle to support the gauze in a perpendicular position, it makes, when the candle is lighted, a vertical section of the flame; and if the flame thus bisected is screened from currents of air, it traces upon the wire-gauze marks of the different actions of its several parts; the edge of the wire-gauze in the weakly luminous exterior flame becomes red-hot and peroxidized; the part next to this, which cuts through the strongly luminous surface, becomes thickly coated with charcoal, which forms a black line within the red one, and corresponds with it in form, which is that of a sugarloaf; within this line the wires are but slightly blackened, and thus mark the space occupied within the flame by the gases and inflammable vapour issuing from the wick.

The above experiment shows that it is the nearly invisible part of the flame which produces the most heat, and in which alone the atmospheric oxygen has any effect on the wire-gauze, and makes it probable that it is the high temperature of this part of the flame which occasions the decomposition of the inflammable vapour and gas in contact with its interior surface, thus producing and igniting the charcoal. It also shows that the principal deposition of the charcoal is not in the interior parts of the flame farthest removed from the air, but principally at the luminous surface, continuing but a very little way within it. The horizontal section by wire-gauze of the flame of a candle screened from currents of air proves the same thing. The charcoal deposited on the wires in this situation is arranged in the form of a ring, and not in that of a black spot, as it has hitherto been described. Further proofs may be obtained, first, by observing that it is only on the summit of a long unsnuffed wick, where its edges come into contact with the luminous surface, and not round the body of the wick, that any deposition of charcoal takes place: and, 2dly, by performing the following experiment: take a glass tube of about two inches long, open at both ends; its external diameter must be less than that of the flame of a candle, and its internal diameter about equal to that of the wick of the same candle; over this wick, previously snuffed, it must be supported in a perpendicular position while the candle is burning; it thus forms a kind of chimney, through which the vapours and gases issuing from the wick partly rise, and which may be set on fire at its upper extremity; when the tube has been for a few seconds in this position, if it be examined, it will be found to be coated with charcoal on its exterior surface, while its inner surface is nearly free from that substance. By conducting the unburned vapour and gases off from the candle in a lateral direction, which may be effected by bending the above-mentioned tube once at a right angle, and having the horizontal portion considerably lengthened, much of the inflammable vapour will condense within the tube, from whence it may be collected and examined. Some that I collected in this manner from the burning wick of a tallow candle had the following properties: its colour was orange-brown; smell, powerful and disagreeable, exactly the same as from a candle just blown out. That which condensed in the hottest part of the tube had the consistence of bees'-wax, and its melting point was about 212°; but that which condensed in the coolest part was much softer, and melted at 90°. When heated considerably, it takes the form of a white vapour; it burns with a white flame; it is insoluble in spirits of wine, but is very soluble in oil of turpentine; it dissolves also in liquid ammonia, and in liquid potash. Nitric acid has but little action on it, even when heated. From this slight examination, it appears to be tallow slightly altered, and rendered empyreumatic, but retaining most of its characteristic properties.

The causes why the light proceeding from a candle is so much diminished by a long unsnuffed wick, while the consumption of the tallow is at the same time increased, do not seem to me to have ever been correctly stated. The only reason I have ever seen assigned for it in chemical works is, that in this situation the fused tallow is carried up to the top of the wick; and the volatile products into which it is there converted not having to pass through so great a length of flame as when the wick is short, are not so completely burned. This explanation is unsatisfactory, because it does not show why the flame does not ascend as high as the volatile inflammable materials supposed to escape unburnt above it; and it is incorrect, for the fused tallow does not rise to the top of a long wick; as may be proved by observing the real height to which it rises, by placing the candle nearly in a horizontal position, when the flame will extend laterally as far towards the end of the wick as the fused tallow proceeds, and leave the rest of the wick unsurrounded by flame. Besides, I find that an equal diminution of light may be produced by lowering a metallic cylinder of the size of the wick from the apex of the flame to the summit of a short wick. In this instance there cannot be any fluid tallow drawn up too high; and some other cause for the loss of light must, therefore, be sought for.

To me it appears that there are two causes operating at the same time in producing the obscuration in question: the one is the opacity of the wick; and the other is its conducting power. In order to have an idea of the effect of its opacity, imagine a room illuminated by rows of lamps disposed round a central black pillar, and conceive what would be the increase of light in every part of the room by the removal of this central pillar, which, from its situation, must have intercepted the rays of light from nearly half the lamps in every direction, and from its colour could not have restored back any of those rays by reflection. If, instead of lamps, a sheet of flame surrounding the pillar be conceived, the analogy to the unsnuffed candle will be still stronger, and the prejudicial influence of the wick be better illustrated. But in order that this explanation should be acceded to, I must first show that the opinion that "flame is an opaque body," is erroneous; for, were it really opaque, it would be of comparatively little consequence whether any solid body occupied its centre or not. Let, therefore, the following experiments be made:—

Light a spirit lamp, and also a tallow candle. Then try to observe the flame of the spirit lamp through that of the candle, and you will find that you cannot succeed. Now reverse the experiment; and you will see clearly the flame of the candle through that of the lamp. Remove now the spirit lamp; and substitute for it another candle. Let both the candles burn until they require snuffing very much; then snuff but one of them; and on trying to look through the upper part of the flame of the one burning brilliantly at the flame of the other burning in a dull manner, you will find the attempt vain: but, by changing the situations of the two candles, you may easily see the bright flame through the dull one.

Thus the supposed opacity of flame is nothing more than this: the rays emitted from a weak light cease to affect the eye while that organ is affected by a stronger one.

But I shall not content myself with proving that flame is not an opaque body, but adduce the following experiment to show that it is a remarkably transparent one:—

Place a lighted spirit lamp about three feet from a sheet of white paper stuck against a wall, and about nine feet from a lighted candle, so that the lamp may be between the wall and the candle. Close by the side of the flame of the lamp put a small piece of thin and clear glass. Then look at the sheet of paper, and two very faint shadows will be seen upon it: the one will represent the flame of the spirit lamp, and the other the piece of glass; but the shadow of the glass will be the deepest: consequently the flame intercepts less light; or, in other words, is more transparent than glass.

The opacity of flame will not now, I presume, be urged against the explanation which I have given of one of the obscuring effects of the wick. I proceed now to mention the other, which depends on its conducting power. A long wick conducts downwards a considerable portion of the heat of the flame, which is expended in volatilizing more tallow (hence the increased consumption of that substance); and by thus diminishing the temperature of the flame, the particles of charcoal are not ignited to such an intense degree as they otherwise would be, and consequently they do not emit so much light. Hence whatever conducts off much caloric from the flame diminishes its light. I have been able to reduce the temperature of the flame of a candle so considerably by placing it in an impure atmosphere, at the same time that a metallic sphere of the size of a musket-ball was suspended so as to dip deep into the flame, that it burned with a feeble blue light, like that of spirits of wine. A similar effect takes place at the bottom of the flame of a candle: the small quantity of gas and vapour emitted from this part of the wick burns here with a feeble blue flame, in consequence of its proximity to the wick replete with tallow passing to the gaseous state, by which its temperature is kept down. In the lighted stream of coal gas the same cooling effect is produced near the bottom by the constant current of cold gas. In all these cases the heat of these parts is not sufficient to decompose any portion of the gas inclosed within the outer flame; consequently no charcoal is separated and ignited; and therefore (conformably to the principle discovered by Sir H. Davy) the emission of light is very trifling.

The preceding observations have led me to attempt an improvement in the construction of lamps for burning oil. Should this attempt succeed to my expectations, I may possibly at a future time lay the result before the public.

With much esteem I remain,

Dear Sir, yours very truly,

Tower, Jan. 28, 1817. R. Porrett, jun.

Article II.

Account of a remarkable Fossil. By Thomas Thomson, M.D. F.R.S.

The fossil which is represented in Plate LXVI., Fig. 1, was found about a year ago in the parish of Alfold, in the county of Surrey, some miles east of Guildford. The part of the country where it occurred is very flat. The petrifaction was met with about eight feet under the surface, in a bed of clay. But over the clay in that particular part is a bed of gravel, which extends to a considerable distance east and west; and varying in breadth from 10 or 12 yards to about a furlong. This bed of gravel is bounded on every side by clay, and has very much the appearance of having been formerly the bed of a river now dried up. The soil over the gravel is much more fertile than that over the clay.

The specimen represented in the figure is nearly square, and about four inches in length, and nearly as much in breadth. Three or four pieces of nearly the same size were found at the same time; but the workmen who discovered them unluckily destroyed them. My specimen was fortunately preserved by falling into the hands of Mr. John Street, of Birtley, to whom I feel myself under great obligations for having politely presented it to me, when I had the good fortune to visit him last autumn.

The mass of the specimen is a very hard clay, the upper surface of which is covered with scales lying in regular order. These scales are thin rectangles, about three-fourths of an inch in length, and five-eighths of an inch in breadth. They have a brownish-black colour; but, when viewed against the light, are a fine hair-brown. Their lustre is shining and silky. Some of them have a semimetallic lustre, somewhat similar to what often appears upon the scales of fish. Most of them, when viewed by candlelight, reflect a lustre somewhat similar to mother-of-pearl. These scales are too hard to be scratched by the nail; but they yield readily to the knife, and have, as nearly as I can determine, the hardness of bone. In many places they are rent or broken in different directions, and the rents are filled with the same clayey matter of which the specimen consists. This clay cement has the appearance of thin veins in the scales. These scales are slightly translucent on the edges. They are very easily frangible. Sectile. Sp. gravity 2·54. When heated they decrepitate, and when kept in a red heat they become white, as is the case with bone.

I kept 2·9 gr. of these scales for half an hour red-hot in a platinum crucible. The weight by this exposure was reduced to 2·57 gr. The portion of scale had become grey, but was not quite white. The 2·57 gr. dissolved with effervescence in nitric acid, and left a small charry residuum, which I could not weigh, but which could

Pl. LXVI.

Fig 1.

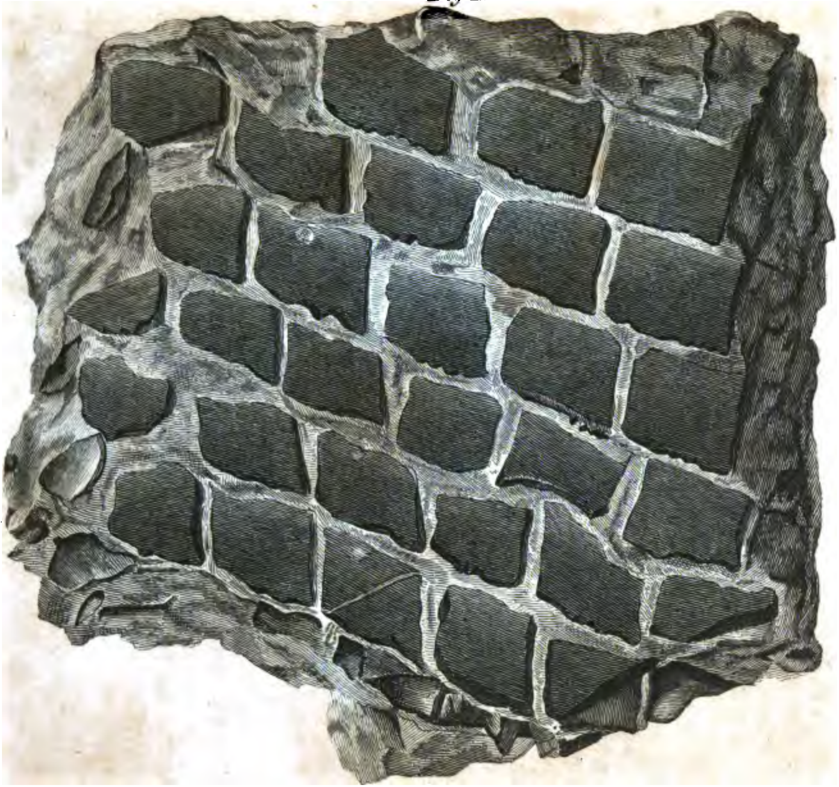
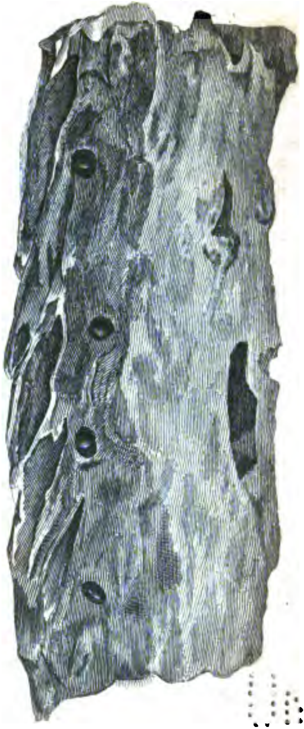


Fig 2.



A Remarkable Foſsil,

Engraved for Dr. Thomson's Annals-for Baldwin, Cradock, & Joy Paternoster Row. May 1. 1817.

not exceed 0·02 grain in weight. The nitric acid solution being supersaturated with ammonia, let fall a white precipitate, which was phosphate of lime, and weighed, after being exposed to a red heat, 1·9 gr. When this phosphate was digested in potash ley it assumed a yellowish-red colour, indicating the presence of a little phosphate of iron. The potash ley being separated by the filter, and mixed with sal-ammoniac, a very slight opalescence took place, indicating the presence of a perceptible quantity of alumina.

The nitric acid solution, being mixed with bicarbonate of potash, let fall a quantity of carbonate of lime, which weighed, when dried, 0·57 gr. Thus the constituents of the portion of scale examined were as follows:—

Animal matter and moisture	0·33
Phosphate of lime, with trace of phosphate of iron and alumina	1·9
Carbonate of lime	0·57
Loss	0·1
Total	2·90

Or, in the hundred parts,

Animal matter	11·37
Phosphate of lime	65·51
Carbonate of lime	19·65
Loss	3·47
	100·00

This analysis is sufficient to show us that the scales are composed of animal matter. They bear a considerable resemblance to bone in their composition, but they contain a greater proportion of carbonate of lime. Perhaps the quantity of carbonate of lime which I state is rather above the truth; for the quantity of it was so small that I could not venture, without exposing it to too great a loss, to dry it in a red heat. It was merely dried upon the filter, and its weight estimated by carefully rubbing it off the filter, and determining the loss of weight which the filter sustained. But as carbonate of lime contains no water of crystallization, and the filter had been exposed for some hours to a heat considerably above 100°, I do not think that any inaccuracy could have crept in from my mode of determining the weight of the carbonate of lime. My original experiments were destined to be made upon 15 gr. of the scales; but this portion was lost from an accident; and I was unwilling to destroy the specimen by detaching any more of the scales. Hence the reason of the small scales upon which my analysis was made; but as I took considerable pains, I consider the preceding result as pretty near the truth.

Fig. 2 exhibits a side view of the fossil, in order to show a set of vessels by which the scales seem to have been supplied with nourishment. These vessels are likewise composed of a bony matter precisely similar in appearance to the scales themselves.

Imbedded in the clay, of which the specimen chiefly consists, there are a considerable number of bodies precisely similar in appearance and composition to the scales themselves; but they are generally smaller, pointed, convex on one side, and having a very distant resemblance to sharks' teeth.

From the experiments made by Mr. Hatchett upon the scales of fish, we learn that their constituents are the same with those which I found in the scales of this fossil. This gives a probability to the conjecture, which one cannot help forming, that the scales of this fossil constituted the covering of some unknown species of fish. This is not the first specimen of the kind which has been met with in Great Britain. In the Phil. Trans. for 1773 (p. 171) there is a figure of a similar fossil, together with a short description of it, by the Hon. Daines Barrington. His specimen was found near Christ Church, in Hampshire. Dr. Woodward, in his Catalogue of English Fossils, describes a still larger specimen of the same kind, found in Stanfield Quarry, near Woodstock. Single scales from the same quarry have been several times met with. I am not aware that any light has been thrown upon the kind of fish to which these scales may have belonged. Perhaps some ichthyologist will have the goodness to inform us whether any fish at present known is covered with scales bearing a close resemblance to those in our fossil.

ARTICLE III.

Some Notices respecting Mercurial Preparations in Use amongst the Chinese. In a Letter from Mr. Pearson, Surgeon, to Dr. Robert Briggs, Chandos Professor of Medicine in the University of St. Andrews.

I HAD long observed that the Chinese apothecaries' shops were supplied with various preparations of quicksilver, and that they furnished nearly as many resources to medical practice derived from that mineral as our own; but to any inquiries respecting the chemical processes by which they were prepared, I could obtain only vague and incorrect answers, it appearing to be no part of the duty or profession of the venders to possess knowledge of that kind. Having procured a person whose occupation it was to prepare some of them, and to dispose of them to the medicine shops, I engaged him to go through the different steps of the processes which he practised, in my presence. For this purpose he brought his materials with him.

In order to prepare a muriate of quicksilver, they were as follows:—

Sulphate of iron, a weight of	920 gr.
Alumine	920
Nitrate of potash (this was very impure)	900
Sulphuret of quicksilver in the state of levigated vermilion	120
Another sulphuret* (also levigated)	120
Muriate of soda	920
Sub-borate of soda	330
Quicksilver	660

An apparatus and vessels for his purpose were readily found on the spot, his furnace being one of the baked clay portable cooking stoves in use amongst the Chinese, of which the furnace part would have been filled by two quarts of fluid, and the ash-pit by a fourth of that measure; also an unglazed earthenware dish, which would contain about a pound; one of similar shape, and rather more than double capacity, of which he had the bottom beaten out; a common flat porcelain plate, and a large earthenware vessel, with some water in the bottom.

Having mixed all the ingredients except the two sulphurets and the quicksilver, without powdering any of them, he put them in the unglazed earthenware dish. He then strewed the two sulphurets over them, and set the dish upon the furnace over a few live charcoal embers. The whole being fused in about half an hour, except the lump of nitre, he added the quicksilver and increased the fire, although still the heat was very moderate. After an hour, and after the ingredients had fused together and blistered up, he removed the vessel (to which the spongy mass adhered) from the fire. This he inverted so as to pour out a portion of the quicksilver (which he returned to the vessel), and placed it upon the fire again. Upon removing it after ten minutes, and when he found upon trial that no quicksilver escaped, he inverted it upon the plate, and heaped up common salt all round the sides of the dish, and also upon its bottom. Over this he inverted the other dish (with the bottom beat out) so that its rims rested upon the edges of the flat plate. Having placed another earthenware dish so as to serve for a stand in the water of the large vessel, he placed the plate upon it, the water then being in contact with all the under part of the plate, but not coming over its edges. He then heaped more salt upon the bottom of the dish, and covered it with a brick, and filled the interstices between the salt and the outer dish with pieces of ignited charcoal. After half an hour he added more charcoal, and urged the fire by fanning, applying his ear from time to time, in order to listen, he said, for a hissing and bubbling noise. This he watched for, and announced the occurrence of with alchemical charlatanery. He said the charcoal already in the furnace might be allowed to burn out, and added, that, in conducting his process during hot weather, it would be proper to draw off the water as it became warm, replacing it by colder: in the then temperature of the atmosphere (48°) it was unnecessary to do so.

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* The exact nature of this I am not aware of. It contains a very large proportion of sulphur, coloured yellow I apprehend by iron. In an incorrect list of the Chinese Materia Medica in my possession, the characters expressing it are employed to denote calamine, and in another place, with a slight variation, the sulphuret of arsenic. It was described to me as being procured in the state I saw it from the earth, and that it was administered in many diseases, especially cutaneous ones, both externally and internally, and held to be perfectly innocuous,

He returned next day, bringing as a standard specimen of the product from the process which he was conducting, the salt which is contained in the vial No. 4, and proceeded to remove the charcoal ashes and the salt, and to lift up the inverted dish. The product was collected upon the plate, some of it white, some discoloured, and also some quicksilver not at all oxidized. That being removed, the whole was collected, and found to weigh 190 gr. The product (as will be seen from the specimen contained in the vial No. 2) bore comparison with the standard preparation but very ill; and he said that, in manufacturing the article for sale, he had no other resource on such occasions but the repetition of the process until he succeeded better than in the present instance he had done. He showed himself to be considerably disappointed by the result of this process, and requested to be allowed to repeat it with his own materials, except the nitrate of potash, which was supplied him. He went through every step of the same process with accurate adherence; and in this instance the experiment succeeded; as from the plate, and the sides of the dish unoccupied by the mixture, by a feather and scraping, two drachms of a white powder mixed with fine needle-like crystals, were removed. This approached the standard pretty nearly, and appeared to be altogether as white and pure as any specimens which I have had occasion to see in or from the shops.

The preparation of the red nitric oxide of quicksilver being also a branch of his business, I requested to be allowed to see the different steps in the process for that likewise. His furnace was the same as before; but his vessel in this instance was a cast-iron pan, of a size proportioned to it, and the description and shape of what obtains the name of a tatch in these countries. Before putting the ingredients into it, he allowed it to have become thoroughly hot by bits of charcoal under it. His ingredients were—

Sulphate of alumine,
Nitrate of potash,
Quicksilver.

Each 1·920 gr. (4 oz.) He fused the first by itself, and added to it the nitrate of potash, and then the quicksilver. His fire was now stronger than in the last process; and after the ingredients had been exposed uncovered to a quick action of it during a few minutes, the operator inverted a glazed earthenware bowl over them, of such diameter as to leave about an inch of the edges of the pan over its rims. He heaped salt round the sides and over the bottom of the bowl, upon which he placed a brick. When nitrous acid vapours began to rise through the salt, he appeared at first desirous to stop their egress by adding fresh salt, after which he paid no further attention to them. By additions of thoroughly ignited pieces of charcoal he kept up a considerable degree of heat under the tatch for upwards of two hours; when, having filled his furnace with pieces of charcoal, he said it might be allowed to burn out, and the vessels to cool. Next morning, when the brick and salt were removed, the nitric oxide was found closely adherent to and crusting the inside of the bowl. When all was scraped off and collected, it weighed 1440 grains.

The object of the following process appeared to be to obtain a sulphuric oxide of quicksilver; but, as the specimen will show, it was but imperfectly fulfilled, owing to the mixture of the oxide which it contains. The ingredients for this preparation were—

Sulphate of alumina	930 gr.
Sulphate of iron	372
Sulphuret of quicksilver	310
Nitrate of potash	2480
The yellow sulphuret before mentioned	310
Quicksilver	930

The nitrate of potash which he used upon this occasion was remarkably well purified. He fused the ingredients in the same kind of vessel described as having been used in the last process thoroughly heated beforehand, by having stood on the fire; and having added the quicksilver, he inverted the glazed bowl over the contents of the pan, and covered it round the sides and over the top with an earthy powder of a red colour, which he brought with him, and which he informed me was the powder of foliated gypsum, and, by having been often employed in the same process, derived its red colour from the fumes which had passed through it. Over the top he placed a brick. A strong fire was kept up under the pan during two or three hours, after which it was allowed to burn out. The apparatus was removed next morning, when the bottom of the bowl was found crusted with a red oxide; the other part, the rims and surface of the matter adherent to the pan, with a yellow one, which he scraped off, and which weighed 680 gr. He did not consider the process as having been successful, from the admixture of red oxide with the yellow one, which it was his object to obtain solely, and was desirous to repeat the process; but as he proposed no variation in the circumstances or conduct of it, I did not trouble him to do so.

For the preparation of another oxide of quicksilver, the following was the process observed. The ingredients were—

Sulphate of alumina	1240 gr.
Sulphate of iron	310
Nitrate of potash	1860
White oxide of arsenic	310
Quicksilver	1240

This also was mixed and fused in the same kind of cast-iron vessel, and the arsenic added after all the other ingredients, when a glazed porcelain bowl was inverted over the tatch. The operator then covering his mouth and nostrils with a cloth, proceeded, as in the last process, to heap the discoloured gypsum round the sides and upon the top of the bowl, over which he placed a brick. The peculiar smell of the arsenical fumes soon became very perceptible and offensive, and continued so during a quarter of an hour. After keeping up a pretty strong charcoal fire during two or three hours, he allowed it to burn out; and, when every thing had cooled, reverted the bowl, of which the bottom part was lined with a red oxide; the rest and rims with a whitish one (in this instance the whole product was expected to have been of a red colour). When collected, the oxide weighed 360 gr. In both the last processes the operator preserved the residua, as, powdered or dissolved in water, they are considered to be useful preparations in the treatment of cutaneous disorders.

I have reason to conclude that, in addition to the preparations specified, the Chinese have the red oxide of quicksilver.

There is another of their mercurial preparations, the only oxide, as far as I can learn, which they ever think of administering internally, and which I believe to be a very general and useful instrument of their medical practice; but respecting the process by which it is prepared, I cannot at present obtain accurate information, as it is said to be properly made in the province of Fo-kien only, whence it comes in small boxes wrapped up in a printed paper.* It is in fine flakes, of a pearly-white colour, and must, I think, have been the preparation which Dr. Black considered to be an oxalate of quicksilver. I have no reason to think that the Chinese can have any acquaintance with the oxalic acid; and they assure me that neither the acetic nor citric are employed in the process. Of the agency of the mineral acids in their processes, or even their existence in an uncombined state, I believe them to be perfectly ignorant. What calomel is in European practice, I conceive the preparation in question to be in theirs, although they pretend to say that its internal use is dangerous, whilst externally, to ulcers and cutaneous affections, it is an efficacious application. I have seen it entered into a Chinese prescription for internal use in a chronic disease; and I believe in siphylitic affections it is very generally administered, although perhaps, in accommodation to common prejudices, disguised from the patient's knowledge.

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* The paper announces the maker's name, adding that he manufactures three preparations (the muriate and nitrate of quicksilver, and the oxide in question) of a superior quality to any other.

I had at one time reason given me to suppose that they possessed a safe and mild submuriate of quicksilver, and a process was spoken of by which it was said to be procured—precipitation from a solution of some of their sublimates by means of salt dissolved in water. The Chinese who thus informed me was employed in European pharmacy, and had acquired sufficient knowledge of the Latin language to peruse pharmacopœias or chemical books written in it. I therefore suspect that his acquaintance with such a process was derived from his foreign knowledge, as I have never since been able to hear of any such preparation amongst them, and his death prevented my following out the inquiry by his means.

As the Chinese are perfectly acquainted with the mode of oxidating quicksilver by triture, it may easily be supposed that they have many variations of mode by which to administer mercury in that form. I believe the most prevalent formula to be by triturating the quicksilver with recent and juicy leaves until it is extinguished. The leaf in which they wrap up betel nut for mastication is here generally made use of, and, with the addition of some other unimportant ingredients, a mass for pills is formed.

It appears, then, that the Chinese are possessed of a variety of active preparations of quicksilver, nearly similar to those which Europeans use; that their processes are greatly more imperfect, unscientific, and uncertain, as to the results than ours, as well as much more expensive.

I apprehend they apply them also to nearly the same practical purposes as we do; and whether for good or evil will depend, still more than amongst ourselves, upon the experience and good judgment of the individual practitioner, owing to the state of medical knowledge amongst them.

With the disease in which the efficacy of the remedy is most complete, they as invariably associate the remedial use of, and necessity for recourse to it, as Europeans can possibly do. It has been stated by very respectable authority that the venereal disease is comparatively rare throughout the Chinese empire, and almost unknown in the interior of it; but I have grounds for believing that it is quite as universally known and spread there as in all other countries; and indeed a people so thoroughly vicious and sensual would be least of any entitled to the privilege of such an exemption. Although not confined to Canton, it may be peculiarly frequent there; but can only be so in the same sense that it is much more prevalent in Portsmouth or the purlieus of Wapping than in Northampton or Inverness, and the Chinese do designate it by an appellation denoting its foreign origin;* but what country has been willing to acknowledge so disreputable a malady as indigenous? They appear to have similar differences of opinion respecting the period of its appearance amongst them with the Europeans; and as we have not made up our minds whether to consider ourselves indebted for the acquisition to the communications with the East which the Mahometan irruptions into Europe, and the pious prowess of the crusadoes brought about, or at a much later period, to the followers of Columbus; so the Chinese have not settled the point, whether the resort of strangers to Canton within these three centuries, or that of the Arabians to different ports on their coast in the eighth or ninth, has brought them acquainted with the disease.

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* The poison of the foreign plants.

As the oxides of which the processes are detailed are too dangerous for internal use, they are employed for their escharotic qualities upon the first appearance of the venereal disease, as if apprised that the symptoms are in the very beginning local only, and that a cure may be effected by destroying them before the continuance of the sores has extended contamination to the system. In as far as I have hitherto learned, the Chinese do not practise mercurial inunction; but fumigation, and that principally, if not exclusively, by burning the sulphuret of quicksilver, is of as universal and immediate application, as the other practice is with us, inducing the peculiar effects of mercury with celerity and violence.

As before mentioned, they also resort to an empyrical use of mercurial preparations in different chronical complaints; and I have no reason to think that they have any other principles of application in such cases than that the diseases have resisted the usual plans of treatment or courses of medicine.

To external and cutaneous affections they seem universally to apply them, and their muriate of quicksilver must for such be a powerful remedy, to which its being of so ready solution is an advantage. A remedy for herpetic eruptions, in much vogue amongst them, appears to be a composition of it mixed with vegetable powders, or with some of the boly scented with musk. This is mixed with warm water, and rubbed upon the affected part. I have recently seen the disease, which I believe it is now agreed to designate by the name of mercurial Gezema, brought on by an unguarded use of this remedy had recourse to for the removal of a slight ring-worm.

Hon. East India Company's Factory,
Canton, March 8, 1815.

A. Pearson.

Appendix.

Dr. Briggs having been so good as to give me about 5 gr. of the white mercurial salt in scales described in the preceding paper (p. 348), I was enabled to make a few comparative experiments with it, which I shall here state. They will serve to facilitate the examination of any person who happens to have a sufficient quantity of it to be subjected to the requisite trials in order to determine its composition.

It was in soft, silky scales. Tasteless; and insoluble in water.

Before the blow-pipe it evaporates rapidly in a white smoke.

When digested in sulphuric acid, no smell of acetic acid is given out. I was induced to try this experiment, because the substance very much resembled acetate of mercury in its appearance.

I digested 3 gr. of it in a solution of 3 gr. of bicarbonate of potash in distilled water. The scales soon disappeared, and a greyish-white powder remained undissolved. The solution was poured off, and the residue edulcorated with distilled water. Diluted nitric acid was poured on the residue. A slight effervescence took place, a portion was dissolved, and a portion remained, having exactly the scaly appearance, colour, and lustre, of the original salt. By repeating this process three times, I decomposed and dissolved the whole matter, except about $\frac{1}{20}$ gr., which I neglected. After the last digestion in bicarbonate of potash, the residue was black. Thus I combined the acid of the salt with potash, and the base with nitric acid.

The nitric acid solution was not affected by infusion of nutgalls; but it was precipitated white by prussiate of potash and by sal-ammoniac. These properties, together with the volatility of the original salt, show clearly that the base of the salt is mercury; and as it is left black by the bicarbonate, we may conclude, I conceive, that the mercury is in the state of protoxide.

The bicarbonate of potash solution (containing the acid) was saturated with acetic acid, evaporated to dryness, and the residue dissolved in distilled water. With this solution, I tried the following experiments:—

Nitrate of lime	Occasioned no change.
Muriate of barytes	A white precipitate, not redissolved by nitric acid, but nearly so.
Nitrate of strontian	A white precipitate.
Sulphate of magnesia	O.
Nitrate of lead	A white precipitate, redissolved by nitric acid.
Nitrate of silver	A white precipitate.
Nitrate of mercury	A white precipitate.
Proto-sulphate of iron	O.
Persulphate of iron	O.
Sulphate of copper	O.

It is clear, from the inefficacy of nitrate of lime, that the acid is not the oxalic. To determine whether it might be the tartaric or citric, I made the following comparative trials:—

Effect of Re-agents on Tartrate of Potash.

Nitrate of lime	A dense flocky-white precipitate.
Muriate of barytes	Ditto.
Nitrate of strontian	Ditto.
Sulphate of magnesia	O.

Nitrate of lead	Dense white gelatinous precipitate, redissolved by nitric acid.
Nitrate of silver	Ditto, but not gelatinous.
Nitrate of mercury	Dense white flocks.
Proto-sulphate of iron	Becomes yellow.
Persulphate of iron	O.
Sulphate of copper	O.

The precipitate formed when nitrate of lime is dropped into tartrate of potash shows that the acid in the Chinese salt is not the tartaric.

Effect of Re-agents on Citrate of Potash.

Nitrate of lime	O.
Muriate of barytes	Copious white flocks, nearly, but not completely, redissolved by nitric acid.
Nitrate of strontian	A copious white precipitate.
Sulphate of magnesia	O.
Nitrate of lead	Copious white flocks, redissolved by nitric acid.
Nitrate of silver	A white precipitate.
Nitrate of mercury	Ditto.
Proto-sulphate of iron	Becomes yellow.
Persulphate of iron	O.
Sulphate of copper	O.

These experiments coincide with the effects of the same re-agents on the acid of the Chinese salt. I should, therefore, have concluded that the salt in question was a citrate of mercury; but I was not able to produce a similar salt, either by digesting oxide of mercury in citric acid, or by decomposing nitrate of mercury by citrate of potash. My citrate was always a powder, and never in scales. I ought to have mentioned that the Chinese salt contains a mixture of gypsum. Probably, therefore, their mode of preparation is complicated: and I had projected some experiments to verify a conjecture respecting the mode which they may have practised; but want of leisure has hitherto prevented me from putting them in execution. Should they be attended with success, I may at some future time lay the results before my readers.—T.

ARTICLE IV.

On the Quantity of Matter having an Influence on Chemical Action.

By M. P. L.

It has been advanced by Berthollet, and supported by some other chemists, that the quantity of matter has an influence on chemical action. Now I think the explanation of those phenomena which these chemists have endeavoured to show to proceed from the above cause, is unsatisfactory, and probably they may be explained on another principle. We find, by taking a compound of two ingredients, A and B, and between which there is a greater affinity than either A or B possess for C, that, by adding C in a small quantity, no decomposition takes place; but by adding C in a larger quantity, it effects a decomposition between A and B, and unites with either A or B. Those chemists who maintain that the quantity of matter has an influence on chemical action explain the above as follows: they say it is owing to the largeness of the mass of C, and that C in this case exerts a greater power in consequence of its mass being increased. Now I would ask, but with all deference to the opinions of those chemists who maintain the above doctrine, if it might not be produced from the following cause? Instead of making use of imaginary bodies, I will take two instances of chemical combination to illustrate my meaning. The first is a resinous gum dissolved in alcohol; the second, antimony dissolved in muriatic acid. If to either of these solutions we add a little water, a precipitation takes place; but immediately upon agitating the mixture, the precipitate is redissolved; by adding a considerable quantity of water, the precipitate is again formed, but will not again be dissolved. It appears to me that it is not owing to any peculiar attraction which the larger quantity of water exerts for the spirit in the first instance, and the acid in the second, that the precipitate is formed; but that the water merely acts as a diluent; the first portion we add dilutes it to a certain degree, but still not so much as to prevent its dissolving the given quantity of resin or antimony; but by diluting it more largely, it becomes incapable of holding the ingredients in solution. We could not dissolve the antimony in the acid largely diluted; and so we may infer that the acid thus largely diluted becomes incapable of holding in solution the portion of antimony which it took up when in an undiluted state. It is, as every chemist knows, a general law, that before chemical action can take place between two or more bodies, they must be in contact. By contact I would be understood to mean the nearest points at which bodies approach to each other; whether they actually touch is a matter of speculation; and with regard to the subject under consideration, it is not necessary to enter on this point. In fact, the particles of the muriatic acid are removed to a distance from the particles of the antimony, and that degree of contact of the two which is necessary for chemical action is destroyed. I conceive that, were it possible to bring the particles of the muriatic acid in contact with those of the antimony, notwithstanding they were in the midst of the water, a union would take place, and no decomposition be formed. It is now generally believed that in combination one atom of this body unites with one, two, or more atoms of the other body. So we may suppose that the water produces a decomposition by removing the necessary number of particles of the muriatic acid from the antimony, which would be requisite for their chemical affinity; say that one particle of antimony unites with two of muriatic acid, the water prevents these numbers of particles coming together.

Article V.

Facts relative to the Influence of Temperature, Mechanical Pressure, and Humidity, on the Intensity of the Electric Power, and on changing the Nature of the Electricity. By J. P. Dessaignes.*

(Read to the French Academy of Sciences, June 24, 1816.)

I have the honour of presenting to the Academy some new facts tending to discover the kind of action which temperature, mechanical action, and humidity, exercise on the electric power, and the manner in which the electricity changes its nature. The experiment of Bergman, of two skeins of silk rubbed together, whose electricity varies according to the kind of friction, is merely an isolated fact, incapable of establishing a principle, because it does not lead to any general consequence. It constitutes a branch which requires to be united to its trunk. If at all times a new fact, though totally unconnected, has excited some interest, surely we cannot refuse our attention to the family to which it belongs.

First Fact.—1. When a glass rod is naturally electric in mercury, if we plunge it into mercury exposed to the external air when the weather is dry and cold, and then bring it near an electric needle, we find it four times more electric than mercury at the common temperature of a close room. To produce this effect, it is not necessary to wait till the mercury be cooled. It is sufficient to carry it into the open air, and then to plunge the glass rod into it immediately. The electric power increases usually on the first impression of the cold. When the rod is naturally unexcitable in the mercury at the temperature of the apartment, and the external air is a little cooling, it is sufficient, in order to render it electric, to carry the mercury out of doors, and to plunge the rod into it.

What I have just said of mercury is common to all the metals.

2. Glass is much less sensible to the impression of cold than mercury. However, if we plunge the rod once into mercury exposed to the open air during a cold north wind, and then immerge it into mercury of the temperature of the apartment, it comes out more electrical than before. When it is naturally unexcitable in mercury, if we cool it gradually with ether, it becomes susceptible of acquiring a very weak electricity, but it becomes again inexcitable when it recovers the temperature of the room.

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* Translated from the Ann. de Chim. et Phys. ii. 59.

Sulphur and sealing-wax are a little less difficult than glass to feel the impression of cold. Paper and cotton, silk and linen, are more so.

Cold then produces and developes the electric power of all bodies; but its influence is not the same for all bodies.

Second Fact.—1. If we plunge a glass rod into mercury during winter, and when it is cold into mercury of the temperature of 32°, it comes out at first much more electric than from mercury of the temperature of the room. If we continue the immersions, we perceive it as it gets cold become more weakly electric, and some time after to become quite inexcitable. It is in vain, then, to continue the immersions; its electricity does not again appear. It refuses equally to become electric when rubbed with flannel. If we allow the mercury and the glass to recover their temperature, the rod gradually recovers its power; and, what is remarkable, when both have recovered the temperature of the chamber, the rod is found to be more electric than before the cooling.

When the electric power is strongly developed, the rod does not lose its energy when plunged into mercury of the temperature 32° till after it has remained 62″. Only 30″ are required when the electric tension is not so strong.

2. The electric power of sulphur and sealing-wax is rather more difficultly destroyed than that of glass by the action of cold. That of silk and of wool is still more so. On Jan. 24, 1814, during a cold north-east wind, wool did not cease to be electric in mercury of 23° till after 20 immersions, remaining 30″ in the mercury at each; that is to say, at the end of six minutes. Eight minutes were requisite for silk, 45″ for sealing-wax, 30″ for sulphur, and 15″ for glass. The electric power of wool cannot be destroyed in mercury of the temperature 26·5°, while that of glass may be destroyed in mercury of the temperature of 43°.

It is remarkable that when glass, sulphur, and sealing-wax, are on the point of losing their energy in cold mercury, they begin to be inexcitable at the bottom of the rod before they are so at the top; while silk and wool lose their energy at the top of the cylinder before they do so at the bottom.

3. If we put a vessel full of mercury into a frigorific mixture of the temperature 23°, and plunge a rod of glass into it at intervals so distant that it may preserve the temperature of the apartment, it comes out for a long time very electric. At the end of about three quarters of an hour its electricity begins to get weak. Some time after the rod becomes inexcitable; though after coming out of that it is always very electric in mercury of the temperature of the room. When the electric tension is very strong, the mercury requires to be cooled down to 14° or 10½° before the rod loses its excitability. When the mercury at this degree of cold is no longer capable of exciting the glass, it is still capable of exciting silk and wool.

In summer, when the mean temperature of the air is high, mercury, and in general all the metals, lose their power by the action of cold more easily than in winter.

It results from this fact that a certain degree of cold weakens and destroys the electric power of all bodies, and that this degree is not the same for all.

Third Fact.—1. On a day when the electric tension is strong, and when the rod is decidedly negative in mercury at the common temperature of the air, if we gradually cool down another vessel of mercury to 32°, and plunge a glass rod into it at each degree which its temperature sinks, it becomes at first more and more negative the lower the temperature sinks. If we continue the immersions, it comes out less and less negative, and at last inexcitable without the electricity changing its nature. If we then allow the mercury and the rod to recover the temperature of the air, the rod becomes again electric, and the electricity is always negative.

The case is different when we preserve the mercury at the temperature of the air, and gradually cool the rod to 32°. It becomes at first less and less negative, then inexcitable, then positive, and at last inexcitable. When we allow it to come to the temperature of the air, it becomes successively positive, inexcitable, and negative, even more negative than at first.

The electricity of the rod, then, remains negative when we weaken the power of the mercury; and it becomes positive when we weaken the power of the glass.

2. When the rod is strongly negative with a north wind, and at a time when the thermometer in the open air is 17½°, if we expose to the open air a glass rod of the size of roll sulphur, and if we take care to carry it into the room every minute, plunging it each time into mercury of the temperature of the room, it comes out at first less negative, then inexcitable. Some time after it appears positive, and again inexcitable. By and by it appears weakly positive, and then finally inexcitable. It is in vain after this to leave it exposed to the cold; no electricity appears when it is plunged into the mercury. When we allow it to recover the temperature of the room, plunging it at intervals into the mercury, we see it becoming successively positive, inexcitable, negative, inexcitable, positive, inexcitable, and at last negative, as before the experiment.

We can at all seasons obtain the same result by cooling the rod by repeated doses of ether.

When the rod is going to change its state, the new electricity begins always at one of the extremities, while the old still subsists at the other. These two electricities are separated from each other by an inelectrical space. It is in such a case that the rod comes out of the mercury sometimes positive at the top and negative at the bottom, sometimes negative at the top and positive at the bottom.

3. When the rod is naturally inexcitable in the mercury, if the mercury and the glass are both without power when rubbed with flannel, and if we cool the rod with ether, it becomes weakly negative in the mercury. If the mercury is still capable of becoming

electric with wool and the rod alone without power, it becomes, on the contrary, positive when it is cooled. In these two cases it is found inexcitable when it returns to the temperature of the air.

The nascent electricity of the rod, then, is negative in the mercury when the latter is naturally without power, and positive when it still preserves power.

4. The electricity produced by friction with wool is equally susceptible of changing its nature when we cool the substance rubbed. I have allowed a large rod of glass to cool in mercury to 14° at a time when it was strongly positive upon rubbing it with wool. After taking it out, I rubbed it from time to time on the sleeve of my coat, as it recovered its temperature. At first it was inexcitable; some time after it was pretty strongly negative; at the end of some minutes it became inexcitable; finally it showed itself positive, and continued so. Sulphur and wax treated in the same way appeared, on the contrary, positive, then inexcitable, and then negative, as usual. When the temperature of the external air is 23° or 21°, we may obtain the same results by exposing these substances to the cold of the atmosphere.

From this fact it results that, if we progressively weaken the power of glass, while we preserve that of mercury at a constant degree of force, the electricity of the rod becomes successively positive, negative, and positive, according to its degree of relative weakness.

Fourth Fact.—Artificial heat produces on the electric power similar effects as artificial cold does.

1. If we gradually heat a glass rod when it is naturally very electric, and at each degree of heat acquired we plunge it into mercury at the common temperature of the atmosphere, it becomes the more electric the more its temperature is elevated above that of the mercury. When we allow it to cool, its electricity again diminishes, and returns gradually to its original state.

The case is different when we heat the mercury, and allow the rod to remain at the temperature of the atmosphere. The rod comes out of the mercury so much the more feebly electrified the higher the temperature of the mercury is elevated above its own. It must be understood, however, that this effect takes place only during the two or three first times that the rod is plunged into the mercury; for if we continue the process, the rod becomes more and more electric in proportion as it becomes hot. Its electricity, however, is much weaker than that which it acquires when it is hot, and the mercury cold.

It seems surprising that hot glass becomes so electric in cold mercury, and cold glass so little in hot mercury. But we must consider that if, in the conflict of the two opposite forces, the one is naturally superior to the other, the effect produced ought to increase when the superior force is increased, and diminish, on the contrary, when the inferior force is augmented.

2. When glass and mercury are naturally inexcitable the one by

the other, if we heat only the rod, frequently the increase of a single degree of heat is sufficient to produce electricity. Frequently 2°, sometimes 4°, and sometimes 8°, are required according to the degree of inexcitability. But when we heat the mercury only, we must raise it 4°, 8°, 16°, or 32°, higher than the rod before electricity is excited by a single immersion.

The electric power of mercury to act on that of glass requires, therefore, four times more force than is necessary for that of glass to act on mercury. Hence the electric power of glass is to that of mercury :: 2 : 1.

Fifth Fact.—1. I have said that on heating the rod its electricity augments progressively. This is the case only to 212°. Beyond that degree the electricity of the rod in the cold mercury diminishes more and more, and disappears entirely at about 410°. When it is inexcitable at this degree of heat, if we allow it to cool, it does not fail to become electric, and to recover nearly its original degree of intensity.

Whatever be the natural tension of the electric power, the cold rod comes out equally inexcitable from the two first immersions into mercury of the temperature 212° or 257°. I say from the first two immersions, for it appears electric at the following immersions as soon as it becomes hot.

We must observe that when the hot rod is inexcitable in cold mercury at 53½°, it is still electric in mercury of the temperature of 140°; and that when the cold rod is inexcitable in mercury of the temperature 257°, a rod heated to 302° does not cease to be electric in it. The inexcitability, therefore, which takes place in these two circumstances is not the result of the destruction of one of the two powers by the heat, but of a momentary equilibrium between the two forces which act against each other; and this equilibrium is equally produced here either by increasing the weaker power, or by giving to the strongest an excessive increase of force.

2. Heat does not always act as a force increasing the tension. In certain circumstances it has the property of weakening it.

During the time of strong tension, if we heat in winter a glass rod so as to make it red hot, and if, after keeping it in that heat for some time, we allow it to cool down to the temperature of the air, it is no longer excitable in mercury, or it is so only very weakly. In summer we may obtain the same result by heating only to 410°. It is not possible in winter to weaken in this manner the power of mercury, because we cannot raise its temperature higher than 347°. But in summer it is only requisite to heat it for an instant to 167° to find it less excitable when cold than it was before.

There is, however, a method of weakening the power by a very small degree of heat even during a time of strong tension in winter.

If we expose for several hours mercury to the cold of the atmosphere when the air is at 32°, and a cold wind blowing; and if we plunge into it now and then a glass rod, it always comes out electric. But its electricity is much weaker towards the end than at the beginning. If we now bring the mercury into a room where the temperature is 46° or 50°, and then plunge the rod into it, we are much surprised to find it inexcitable. On exposing the mercury again to the open air, it speedily recovers its power, and loses it again when brought into the room.

If we cool a glass rod to 32°, or lower, at a time when it is naturally very electric, and then bring it suddenly to the fire to raise its temperature to 86° or 140°, we find it, when cooled again, inexcitable in mercury; or if it is still electric, its electricity has changed its nature. This shows that its power has no longer the same ratio of force to that of mercury. This diminution does not take place when we allow it to return of itself to the temperature of the air. In that case it is more electric than before the cooling.

All that I have said of glass in this fifth fact and in the fourth is common to sealing-wax, sulphur, silk, and wool, some trifling differences excepted.

Thus we may say that, if the first effect of heat is to increase the tension of the electric power, the second and subsequent effect is to repress the expansive force by bringing it nearer the centre of activity of the attraction; and this effect is produced with so much the more facility the rarer the fluid is.

Heat, then, acts on the electric power in the way of attraction by opposing its expansion; and cold in the way of an expansive force by favouring its developement.

Sixth Fact.—These different impressions of heat do not merely modify the tension of the electric power; they change, likewise, the nature of the electricity.

1. If we gradually heat the rod to 212°, when it is strongly negative in mercury, and at each degree of elevation plunge it into mercury at the temperature of the atmosphere, it always comes out more and more negative. If we gradually heat the mercury in its turn, and plunge the rod (at the temperature of the air) into it at every 10° of elevation, it comes out, on the contrary, inexcitable at 176°, and positive when the temperature rises to 212°. If we allow it to get hot in the mercury, it becomes again negative.

2. When the rod heated to 212° is strongly negative in the cold mercury, if we continue to heat it, its negative state diminishes by little and little. At 410°, or about that temperature, it becomes inexcitable in winter. Beyond that degree it comes out positive, beginning at the bottom of the rod. If we raise the temperature still higher, the rod becomes inexcitable, and does not recover its electric state again. In summer, after having become positive, it passes a second time to the negative state; nor is it necessary to heat it so much as in winter.

When the cold rod comes out positive from mercury at 212° at the first immersion, if we continue to heat the mercury, and always keep the rod at the temperature of the air, it becomes inexcitable, and continues so till the mercury is heated to the boiling point. Mercury, then, heated to 317° has not sufficient force to make the rod pass to another electric state; nor is it possible to increase it further. But if we cannot augment its force by heat, we can increase it relatively by weakening that of glass by cooling it. We then see the rod becoming successively negative, inexcitable, and positive.

Thus by increasing the power of the glass the rod becomes successively negative, positive, and negative, according to its degree of relative force; and by increasing that of the mercury, or, which comes to the same thing, by diminishing that of the glass, it becomes successively positive, negative, and positive, according to its degree of relative weakness.

3. The rod passes successively through all these states in the course of the year, when its power begins to appear. It is in winter that it appears nascent, successively positive, negative, and positive, according to its degree of relative feebleness; and it is in summer that it is successively negative, positive, and negative, according to its degree of relative force.

When the nascent electricity of the rod in mercury commences by being positive, it becomes inexcitable and negative by heating it a few degrees. When it commences by the negative state, it becomes successively positive, inexcitable, and negative, in proportion as we heat it. When it appears by the second state positive, we make it become by heating it successively negative, inexcitable, positive, inexcitable, and negative.

Whatever is the state of the nascent electricity of the rod in winter, it comes out of mercury of the temperature 140°, or higher, almost always inexcitable after the first immersion. Then, in proportion as it acquires heat, it becomes electric, and changes its electricity once or oftener, according to the state from which it set out.

If we gradually heat the rod in summer, when its nascent electricity in mercury appears in the first state negative, it becomes successively positive, inexcitable, and negative. When it commences by the positive state, it becomes negative on heating it, and does not go further. When it commences by the second state negative, it may become inexcitable by heating it; but it stops there.

When the nascent electrical state of the rod is negative in summer, it always comes positive out of mercury heated to 140°, and it becomes negative on heating it. If it is naturally positive, it comes out negative, and it remains in that state, on heating it, till the end of its cooling, when it appears very weakly positive before becoming inexcitable.

Thus in developing the powers the electricity of the rod changes its nature once, twice, or thrice, according to the respective state of the forces at the moment of development; and in all cases it terminates by being negative when the two powers are equally developed.

4. The two powers may be inexcitable the one by the other, though each may be rendered weakly electrical by friction with

wool. In this case the rod, being heated some degrees, and plunged into cold mercury, always becomes negative. The same rod being cold, and plunged into mercury heated a few degrees, becomes, on the other hand, positive. The hot rod, after having been negative in cold mercury, is still susceptible of passing into the positive state when heated to a higher temperature.

There are, then, two positive states: the one takes place when the power of the glass is inferior to that of the mercury, and the other when the power of the glass enjoys too great a superiority over that of the mercury. As this last positive state is preceded, as well as the first, by a momentary equilibrium of forces, it can only be the effect of a new weakening of the power of the glass produced by the immersion, by its too great expansion compared to that of the mercury, and by the subsequent revulsion of the last. The positive state, then, is the partition of the most feeble power, or of that which becomes so by the result of the pressure.

5. When the hot rod is decidedly negative in the mercury, if we keep it a long time in that degree of heat, it passes to the positive state as soon as its power begins to get weak, and it is found almost inexcitable when it has cooled.

Seventh Fact.—Mechanical pressures are capable of producing the same changes in electricity.

1. If we merely touch the surface of mercury with the convex end of a large stick of sealing-wax, polished glass, or sulphur, we generally draw it back positively electric. If we strike the surface of the mercury slightly with it, we render it inexcitable: if we strike with greater force, we make it negative. This experiment may be repeated as often as we please.

2. When the powers are well developed, a rod of glass comes equally negative out of mercury put into a very flat vessel as out of mercury contained in a conical vessel. When the power of the glass begins first to get weak in winter, the rod comes positive out of the shallow vessel, and negative out of the deep one. On the contrary, when the power of the glass begins first to get weak in summer, it comes negative out of the shallow, and positive out of the conical vessel. In the same circumstances we obtain the same results with two rods of glass of unequal thickness, and plunged each to the same depth in the same conical vessel.

3. If we press a rod of glass over its whole length, so as neither to communicate heat nor moisture, and then plunge it into mercury, it becomes electric in it even when naturally inexcitable. If it is naturally positive it becomes negative, and it becomes more strongly negative than before when it is naturally negative.

If, instead of pressing the rod, we merely wrap round it a dry linen cloth in different folds, and while we hold the cloth round it draw out the rod gently as from a case, it becomes positive in the mercury after two or three such frictions, when it is naturally negative: and if it is naturally positive, it becomes inexcitable.

4. When a large rod is decidedly negative in the mercury, if we

make a firm ligature on it with tape from its upper extremity to within 2¼ inches of its lower end, and in this state plunge it into mercury, the naked part of the rod comes out at first inexcitable, and some time after more or less weakly positive. On undoing the ligature we find the rod instantly strongly negative in the mercury.

So that when we apply pressure to a part only of the rod, the fluid of the part not pressed acquires expansion.

5. However strong the tension of the power may be, if we press the rod with vigour, and for a long time, along its whole length, it at last loses its power. It is on this account that when we keep it plunged in mercury for a longer or shorter time, when it is not very strongly negative it becomes gradually inexcitable, then positive, and at last finally inexcitable.

Eighth Fact.—Electricity likewise changes its nature under the influence of the principle of humidity.

When the rod is decidedly negative in mercury during dry and cold weather, if we plunge it into water, and, after drying it with a linen cloth, plunge it again into mercury, it comes out positive. If we moisten it again, spreading the water over every part of the surface with the finger, and, after drying it, plunge it into mercury, it comes out inexcitable. In proportion as the moisture evaporates, either by leaving it in the air, or by moistening it with ether, and allowing it to evaporate, it becomes successively positive, inexcitable, and negative, as before the experiment.

We must observe that in this case the power is only weakened when the moisture has not been applied to the whole surface, and that in the contrary case it is destroyed.

It results in general from these facts that the different electric states of glass in mercury are the different effects of two powers whose forces are variable, and frequently change their ratio to each other. In the mutual pressure of these powers it is the one which is most powerful at the instant of their reaction which is always negative, and it is the weakest which is positive. We increase the electric intensity by weakening to a certain point the weakest power, or by increasing in the same way the strongest power; because in both cases we increase the ratio between the forces. On the other hand we diminish the electrical intensity by weakening the strongest power, or by increasing the weakest; because in these two cases we diminish the ratio between the forces. The two powers are inexcitable the one by the other when the two forces are to each other in a ratio of equality or in equilibrium. Finally, the electricity changes its nature when the ratio of the forces becomes inverse; that is to say, when the weakest power becomes superior to the strongest.

ARTICLE VI.

Researches respecting the Laws of the Dilatation of Liquids at all Temperatures. By M. Biot.

(Read to the Societé d'Arcueil, Aug. 3, 1813.)

THE knowledge of the laws of the dilatation of liquids is necessary in numerous chemical and physical investigations. We must be acquainted with the dilatations of water to reduce the specific gravities observed in this liquid to comparable terms. We must be acquainted with those of alcohol to determine its density at different temperatures, or to make use of the thermometers in which that substance is employed. If we endeavour to compare theoretically the dilatability of different liquids with each other, and to combine their greater or smaller dilatations with their tendency to boil or become solid at lower or higher temperatures, we shall find it impossible to do it generally, or even to form precise notions on the subject, until we have expressed the dilatations by general formulas which represent them at all temperatures, and which lay before us the peculiarities of each liquid which we wish to examine.

Such is the object of this essay. I shall show that for all liquids whose dilatations have been hitherto examined the rate of the dilatation may be represented at every temperature by an expression of this form:—

$$\delta_t = a\,t + b\,t^2 + c\,t^3,$$

in which t denotes the temperature in degrees of the mercurial thermometer, and a, b, c, constant coefficients which depend upon the nature of the liquid. I suppose here that δ_t is the true dilatation for unity of volume reckoned from the temperature of freezing water; but it is easy to see that the apparent dilatation follows similar laws; for if we represent the apparent dilatation by Δ_t, and denote by K the cubic dilatation of the vessel in which we observe the liquid,* we have in general

$$\Delta_t = \delta_t - \mathrm{K}\,t,$$

at least if we neglect the square of the coefficient K, which may be almost always done, as the dilatation of solid bodies is extremely small.

Let us suppose that the primitive volume of the liquid, being 1 when $t = o$, occupies at $+ t$ degrees a number of divisions, X, in the vessel whose cubic dilatation is K. This number of divisions will correspond with a greater capacity than when t was equal to o. It will correspond to the capacity

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* What I mean by cubic dilatation here is the triple of the linear dilatation.

$$X \left\{1 + K t + \frac{K^2 t^2}{3}\right\};$$

and as by supposition it is equal to $1 + \delta_t$, since δ_t is the true dilatation for unity of volume, we have

$$X \left\{1 + K t + \frac{K^2 t^2}{3}\right\} = 1 + \delta_t.$$

This gives us

$$X = \frac{1 + \delta_t}{1 + K t + K^2 t^2} = 1 + \frac{\delta_t - K t - K^2 t^2}{1 + K t + K^2 t^2}.$$

The first term of this expression is the primitive volume at 0°; the second is the apparent dilatation Δ_t. We have, therefore,

$$\Delta_t = \frac{\delta_t - K t - \dfrac{K^2 t^2}{3}}{1 + K t + \dfrac{K^2 t^2}{3}}$$

The term affected by $\frac{K^2}{3}$ is absolutely insensible in the most exact observations on the dilatations of liquids made in glass vessels between the temperatures of − 15° and + 100°. If we neglect it, we have

$$\Delta_t = \frac{\delta_t - K t}{1 + K t},$$

a value which, neglecting the square of K, and the product of K by δ_t, becomes

$$\Delta_t = \delta_t - K t,$$

as we have supposed above.

To establish the preceding law, and determine the coefficients *a*, *b*, *c*, relatively to different liquids, I shall make use of a set of experiments made with great care by Deluc on the dilatation of nine liquids, with which he had constructed thermometers, which he regulated in freezing water and boiling water, marking 0° at the first point, and 80° at the second, and dividing the interval into 80 equal parts.* It is true that some of the substances which he employed boil in the open air at temperatures below that of boiling water; but this was not the case in his thermometers, because he had perfectly freed them from air. Rectified alcohol, which boils in the open air at about 165°, when freed from air, and inclosed in a close tube, may be heated to 212° without boiling, and it connues still to acquire heat and to dilate even at that point and beyond it. It is easy to see the reason of this phenomenon from Dalton's theory of the formation of vapours; but here I shall satisfy myself with considering it as a fact. The following table exhibits Deluc's experiments:—

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* Recherches sur les Modifications de l'Atmosphere, tom. 1.

Mercury.	Olive oil.	Essential oil of camomile.	Essential oil of thyme.	Water saturated with common salt.	Strong alcohol.	One part alcohol and one part water.	One part alcohol and three parts water.	Water.
80	80	80	80	80	80	80	80	80
75	74·6	74·7	74·3	74·1	73·8	73·2	71·6	71·0
70	69·4	69·5	68·8	68·4	67·8	66·7	62·9	62·0
65	64·4	64·3	63·3	62·6	61·9	60·6	55·2	53·5
60	59·3	59·1	58·5	57·1	56·2	54·8	47·7	45·8
55	54·2	53·9	53·3	51·6	50·7	49·1	40·6	38·5
50	49·2	48·8	48·3	46·6	45·3	43·6	34·4	32·0
45	44·0	43·6	43·4	41·2	40·2	38·4	28·4	26·1
40	39·2	38·6	38·4	36·3	35·1	33·3	23·0	20·5
35	34·2	33·6	33·5	31·3	30·3	28·4	18·0	15·9
30	29·3	28·7	28·6	26·5	25·6	23·9	13·5	11·2
25	24·3	23·8	23·3	21·9	21·0	19·4	9·4	7·3
20	19·3	18·9	19·0	17·8	16·5	15·3	6·1	4·1
15	14·4	14·1	14·2	12·8	12·2	11·1	3·4	1·6
10	9·5	9·3	9·4	8·4	7·9	7·1	1·5	0·2
5	4·7	4·6	4·7	4·2	3·9	3·4	1·0	−0·4
0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0

Now if we represent by D_T the number of degrees indicated by each of these thermometers, when T is the number indicated by the mercurial thermometer divided into 80 parts, we may represent all these experiments by the general formula

$$D_T = A\,T + B\,T^2 + C\,T^3$$

A, B, C, being arbitrary constant quantities, different for each liquid, the values of which for the liquids observed by Deluc are given in the following table :—

Liquids.	Values of the coefficients.		
	A	B	C
Mercury	+1·000000	+0·0000000	+0·000000000
Olive oil	+0·950667	+0·0007500	−0·000001667
Essential oil of camomile	+0·920442	+0·0013056	−0·000003889
Essential oil of thyme	+0·949335	−0·0001667	+0·000010000
Water saturated with salt	+0·820006	+0·0020275	+0·000002775
Strong alcohol................	+0·784000	+0·0020800	+0·000007750
One part alcohol and one water	+0·705333	+0·0027500	+0·000011667
One part alcohol and three wat.	+0·010333	+0·0155277	−0·000039444
Pure water	+0·160000	+0·0185000	−0·000050000

To show the agreement of these results with observations, I have calculated the value of D_T by the formula for each of the liquids from 20° to 10°, and I have compared them with the numbers observed by Deluc. This is the object of the following tables:—

Calculation of the Olive Oil Thermometer from the Formula

$$D_T = 0{\cdot}950667\,T + 0{\cdot}00075\,T^2 - 000001667\,T^3$$

Liquid.	Degrees of mercurial therm. T	Degrees of the olive oil thermometer.		
		Calculated.	Observed.	Differences.
Olive oil.	80	80·00	80·0	0·00
	70	69·64	69·4	−0·24
	60	59·37	59·3	−0·07
	50	49·20	49·2	0·00
	40	39·12	39·2	+0·08
	30	29·15	29·3	+0·15
	20	19·30	19·3	0·00
	10	9·58	9·5	−0·08
	0	0·00	0·0	0·00

M. Deluc at different times put the olive oil thermometer into a freezing mixture which sunk the mercurial thermometer to −14°; and he says that the oil thermometer stood nearly at the same degree as long as the oil continued liquid. This result agrees with our formula; for if we suppose $T = -14$, the formula gives $D_T = -13{\cdot}21$.

But when the oil began to solidify, the oil thermometer sank all at once much more than the mercurial thermometer. The oil sank altogether into the bulb. It was obviously the congelation that produced this sudden effect; for, after it had taken place, if the temperature was elevated, the mercurial thermometer began to rise immediately; but the oil thermometer remained stationary for a considerable time; no doubt the time necessary to liquify the oil. When once melted, the oil speedily rose as high as the mercury, and continued its usual dilatation. Deluc supposes that it was the absence of air which enabled the oil to acquire so great a degree of cold without freezing, though it would have congealed in the open air; but it appears from the experiments of Sir Charles Blagden that neither the exclusion of air nor rest are absolutely necessary for this effect, though they may contribute to it.

We see by these phenomena, 1. That olive oil in certain circumstances may be cooled down much beyond its ordinary degree of congelation without freezing. 2. That it diminishes in bulk, like mercury, when it freezes. This is quite obvious, as the frozen portions sink to the bottom of the vessel. 3. That to the very moment of its becoming solid it continues exactly, or very nearly, to contract according to the usual law. This appears to be the case with mercury, from Mr. Cavendish's discussion respecting Hutchin's experiments at Hudson's Bay.

We see from this that the oil in cooling to any degree whatever cannot have an apparent maximum condensation (as is the case with water), at least in glass tubes. This is shown by our formula; for this maximum would take place when

$$\frac{d\,D_T}{dT} = 0$$

This gives us

$$0 = 0{\cdot}950667 + 0{\cdot}0015\,T - 0{\cdot}000005\,T^2$$

an equation the roots of which are

$$T = -311{\cdot}1^\circ;\ T'' = +611{\cdot}1^\circ$$

That is to say, that if the oil could remain liquid at these temperatures, and continued to dilate according to the same law, it would have an apparent maximum of condensation when cooled down 311·1 below 0, and an apparent maximum of dilatation when heated to 611·1 above zero. But these points are too much beyond the limits of the observations on which our calculations are founded to warrant the extension of the formula to them. We may conclude that olive oil, as long as it remains liquid, continues to condense by cooling, and that it freezes without dilating, as is confirmed by observations.

Let us now proceed to the essential oil of camomile. For it we have

$$D_T = 0{\cdot}9204416\,T + 0{\cdot}0013056\,T^2 - 0{\cdot}000003889\,T^3$$

The following table shows the agreement of the results of the formula with observation:—

Liquid.	Mercurial thermometer. T	Degrees of the oil of camomile thermometer.		
		Calculated.	Observed.	Difference.
Essential oil of camomile	80	80·00	80·0	0·00
	70	69·49	69·5	+0·01
	60	59·09	59·1	+0·01
	50	48·80	48·8	0·00
	40	38·66	38·6	−0·06
	30	28·68	28·7	−0·02
	20	18·90	18·9	0·00
	10	9·30	9·3	0·00
	0	0·00	0·0	0·00

We see that in this case the formula is as exact as observation itself. It follows in this case, likewise, that the oil has no maximum of condensation; for the equation of this maximum is

$$0 = 0{\cdot}9204416 + 0{\cdot}002612\,T - 0{\cdot}000011667\,T^2$$

the roots of which are

$$T = -\,189^\circ,\ T'' = +\,413^\circ$$

values too remote from our experiments to induce us to consider them as applicable. We see that this oil, likewise, congeals without dilating.

Let us examine in the same way the essential oil of thyme. For it we have

$$D_T = 0{\cdot}949336\,T - 0{\cdot}0001667\,T^2 + 0{\cdot}0000\,T^3$$

The following table shows the agreement of the formula with experiments:—

Liquid.	Mercurial therm. T	Oil of thyme thermometer.		
		Calculated.	Observed.	Difference.
Essential oil of thyme.	80	80·00	80·0	0·00
	70	69·07	68·8	−0·27
	60	58·52	58·3	−0·22
	50	48·30	48·3	0·00
	40	38·35	38·4	+0·05
	30	28·60	28·6	0·00
	20	19·00	19·0	0·00
	10	9·48	9·4	−0·08
	0	0·00	0·0	0·00

Here the agreement is very satisfactory. There is no maximum of condensation; for the equation which that maximum would give is

$$0 = 0{\cdot}949336 - 0{\cdot}0003334\,T + 0{\cdot}00003\,T^2$$

the two roots of which are imaginary. Hence that oil will congeal, like the last, without dilating.

We now come to water saturated with common salt. For it we have

$$D_T = 0{\cdot}820006\,T + 0{\cdot}0020275\,T^2 + 0{\cdot}000002775\,T^3$$

The comparison of this formula with observation is as follows:—

Liquid.	Mercurial therm. T	Salt water thermometer.		
		Calculated.	Observed.	Difference.
Water saturated with common salt.	80	80·00	80·0	0·00
	70	68·29	68·4	+0·11
	60	57·10	57·1	0·00
	50	46·42	46·6	+0·18
	40	36·22	36·3	+0·08
	30	26·50	26·5	0·00
	20	17·23	17·3	+0·07
	10	8·41	8·4	−0·01
	0	0·00	0·0	0·00
	−10	−8·00	−8·0	0·00

The agreement of observation with the formula is as exact as could be desired. In this case the contraction was observed below zero by means of freezing mixtures, and we see that calculation agrees with experiment at that point likewise. This solution congeals, also, without dilating; for the equation of the maximum is

$$0 = 0·820006 + 0·004055\ T + 0·000008325\ T^2$$

the two roots of which are imaginary. Hence water saturated with common salt loses the property of dilating before it becomes solid. It would be interesting to verify this result by experiment;* for, though it is founded on a strong analogy, since the law of dilatation holds at − 10° Reaumur, yet it can be considered only in the light of a very probable hypothesis. But to make the experiment exactly, the thermometer should be completely freed from air, and it should be cooled slowly, that the liquid may remain fluid even when below the freezing point.

Sir Charles Blagden made an observation of this kind, of which an account is given in his interesting memoir on the degree of the congelation of water; but the solution which he employed was not saturated. It contained 4·8 parts of water for one of salt. Consequently its congelation took place at − 10·37°, according to the law which Blagden discovered. This solution contracted in cooling to − 6·67°; but when cooled down to − 7·55°, it appeared to dilate sensibly. These limits are considerably above − 10°, at which Deluc tried the saturated solution which formed his thermometer. Hence the experiment of Blagden cannot in the least injure the law which we have found for the solution which Deluc employed. It is very likely that a certain proportion of salt deprives water of the property of dilating before congelation, and that a

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[*] Such an experiment could hardly be made, as water parts with its salt in the act of congealing.—T.

smaller proportion does not produce this effect. This happens with mixtures of water and alcohol, as will be seen hereafter.

I proceed to highly rectified alcohol. The alcohol used by Deluc was sufficiently strong to set fire to gunpowder, over which it was burned. This is the way in which Deluc characterizes it. He found that, though not quite so strong, provided the proportion of water in it was small, the same results nearly were obtained. The following is the formula for this alcohol:—

$$D_T = 0{\cdot}784\,T + 0{\cdot}00208\,T^2 + 0{\cdot}00000775\,T^3$$

Its agreement with observation will be seen by the following table:—

Liquid.	Mercurial therm. T	Alcohol thermometer.		
		Calculated.	Observed.	Difference.
	80	80·00	80·0	0·00
	70	67·73	67·8	+0·07
	60	56·20	56·2	0·00
	50	45·37	45·3	−0·07
	40	35·09	35·1	+0·01
	30	25·60	25·6	0·00
	20	16·57	16·5	−0·07
	10	8·05	7·9	−0·16
	0	0·00	0·0	0·00
	−10	−7·64	−7·7	+0·06

We see that the calculated quantities agree with those observed, and this holds below zero as well as above it. The law of the dilatation of this alcohol does not indicate a retrogradation. For supposing D_T a maximum, we obtain the following equation:—

$$0 = 0{\cdot}784 + 0{\cdot}00416\,T + 0{\cdot}00002325\,T^2$$

the two roots of which are imaginary.

The value of D_T given by our formula will be very convenient for determining the correspondence of the mercurial and alcohol thermometers. We see that such a calculation is indispensable; for there is still a great difference between the two thermometers, though they agree at the freezing and boiling points. The difference becomes less if the alcohol thermometer is regulated on that of mercury at low temperatures. If we make $T = (T) + T'$, and determine (T) so as to make the square of T' to disappear from D_T we obtain $(T) = -\,89{\cdot}463°$ Reaumur. Then the value of D_T becomes

$$D_T = -\,58{\cdot}981° + 1{\cdot}34218\,T' + 0{\cdot}00000775\,T'^3$$

The alcohol thermometer, then, would stand at $-\,58{\cdot}981°$ on its own scale when T' is null; that is to say, when the mercurial thermometer stands at 89·468° below zero. But setting out from this term, and proceeding to 80° above or below zero, the rate of the two thermometers would be nearly proportional; for the term T'^3, which alone alters the exactness of this proportionality, will not amount to four degrees in the extreme case when $T' = \pm 80°$. Such, then, is the greatest agreement which can ever exist between the alcohol and mercurial thermometers, supposing them prolonged indefinitely below zero.

Let us now consider the mixtures of alcohol and water. When the proportion of water is inconsiderable, the affinity of the alcohol for it will keep it long fluid, and oppose all retrogradation. This is proved by the observations made with the thermometer filled with a mixture of equal parts of alcohol and water. The formula in that case is

$$D_T = 0·705333\ T + 0·00275\ T^2 + 0·000011667\ T^3$$

The calculations from this formula compared with experiment give us the following table:—

Liquid.	Mercurial therm. T	Weak alcohol thermometer.		
		Calculated.	Observed.	Difference.
A mixture of one part alcohol and one part water.	80	80·00	80·0	0·00
	70	66·85	66·7	−0·15
	60	54·74	54·8	+0·06
	50	43·60	43·6	0·00
	40	33·36	33·3	−0·06
	30	23·96	23·9	−0·06
	20	15·30	15·3	0·00
	10	7·33	7·1	−0·23
	0	0·00	0·0	0·00

We see that the law of dilatation is very well represented by the formula. The proportion of water is not sufficient to communicate its own retrograde motion to the alcohol; for the equation, when D_T is a maximum, is

$$0 = 0·705333 + 0·0055\ T + 0·000035\ T^2$$

the two roots of which are imaginary.

But when we increase the proportion of water, the influence of that liquid becomes sensible. This is proved by the thermometer filled with a mixture of one part alcohol and three parts water. In that case the formula is

$$D_T = 0·010333\ T + 0·0155277\ T^2 - 0·000039444\ T^3$$

Here the term proportional to the temperatures is almost insensible. It would only give 0·8° at the temperature of 80°. This is the result of the influence of water; for in the case of pure water this term is negative, as we shall see immediately. The following table exhibits the comparison of experiment with the formula :—

Liquid.	Mercurial therm. T	Weak alcohol thermometer.		
		Calculated.	Observed.	Difference.
A mixture of one part alcohol and three parts water.	80	80·00	80·0	0·00
	70	63·24	62·9	−0·34
	60	47·99	47·7	−0·29
	50	34·40	34·4	0·00
	40	22·72	23·0	+0·28
	30	13·21	13·5	+0·29
	20	6·10	6·1	0·00
	10	1·61	1·4*	−0·21
	0	0·00	0·0	0·00

Here the law of dilatation is very different from what it was in pure alcohol, and the difference between the two thermometers is also much more considerable. Here there is a maximum of condensation; for the equation which is obtained when D_T is a maximum is

$$0 = 0\cdot010333 + 0\cdot0310554\, T - 0\cdot000118333\, T^2$$

the roots of which are

$$T' = -\,0\cdot333^\circ;\ T'' = +\,263^\circ$$

The first of these only is admissible. It gives a maximum of condensation at one third of a degree of Reaumur below zero. If we substitute this value of T' in D_T, we obtain $D_T = -\,0\cdot0017^\circ$; that is to say, that at the instant of this maximum the thermometer filled with the mixture of alcohol and water ought to be sensibly at 0 of its own scale. Accordingly, if we cool it down further, we will see it rising above that point. This maximum is indicated by the weak dilatation of the mixture, which according to observations was only at 0·1° on its own scale when the mercurial thermometer was at + 5°. According to the formula, it ought then to be at + 0·4°. The error is of that kind which may be ascribed to errors of observation.

We come now to examine the law of dilatation in the thermometer filled with pure water freed from air. In this case the formula is

$$D_T = -\,0\cdot1600\, T + 0\cdot01850\, T^2 - 0\cdot00005\, T^3$$

Here the term proportional to the temperature is negative.

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* M. Biot, in a note, observes that this figure in Deluc's book is illegible; but that he conceives it to be 5. In my copy (p. 320), the 4to. edition of Geneva, 1772, it is very legible, and undoubtedly 4.—T.

Among the liquids examined, water is the only one which exhibits this circumstance. Hence we ought to expect that its dilatations would differ much from those of mercury. The following table shows that this is the case :—

Liquid.	Mercurial therm. T	Water thermometer.		
		Calculated.	Observed.	Difference.
Water.	80	80·0	80·0	0·0
	70	62·3	62·0	−0·3
	60	46·2	46·8	−0·4
	50	32·0	32·0	0·0
	40	20·0	20·5	+0·5
	30	10·5	11·2	+0·7
	20	3·8	4·1	+0·3
	10	0·2	0·2	0·0
	5	−0·343	−0·4	−0·057
	0	0·0	0·0	0·0

This thermometer is certainly the most irregular of all; and this is peculiar to water, as Deluc has several times observed in his work. Yet we see that the observations oscillate round the formula within very narrow limits. If we had only these observations to consider, we might make them agree a little better with the formula, by introducing a considerable change in the coefficients. But in that case we would not represent so well other phenomena which we shall notice immediately. Besides, the deviations observed are such as may very well be ascribed to the observations themselves.

Here we have again a maximum of condensation, and it occurs at a more elevated temperature than in the preceding experiments. The equation which determines it is

$$0 = -0·16 + 0·087\ T - 0·00015\ T^2$$

the roots of which are

$$T' = +4·402°;\ T'' = +251$$

The first is that which makes D_T a minimum, and which consequently indicates a maximum of condensation. Deluc says that this maximum appeared to him to correspond nearly with the temperature of + 4°, which differs very little from our calculus.* He says, likewise, that at the time of this phenomenon the water thermometer was about half a degree below zero on its own scale. We find by our formula − 0·35°. This maximum, however, is only apparent, and requires a correction in order to obtain the real maximum. The experiment was made with distilled water freed from air. Common water, containing air, probably dilates in proportions a little different.

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* I cannot here avoid noticing the impropriety of drawing such conclusions from mathematical formulas, entirely empyrical, and founded merely on observations. They may be employed with advantage to facilitate the application of experimental results. This constitutes the real value of the present paper. But to attempt to deduce from them the temperature at which the density of water is a maximum, as Biot does here, is an abuse of mathematics which ought to be carefully guarded against. We cannot discover a new fact by mathematics; but merely demonstrate those found out by other means.—T.

I shall now deduce from these results the true and absolute dilatations of the liquids observed by Deluc. In the first place I shall remark, that the thermometrical observations which we have employed are, in all probability, not exempt from small inaccuracies. Deluc, in the work in which these experiments occur, treats at great length on the construction of the thermometer; but he takes no notice of the necessity of plunging both the bulb and the liquid column into the medium the temperature of which we wish to communicate. The same thing ought to be done in observing the intermediate temperatures between the fixed points. If these precautions were neglected by Deluc, which however is not probable, all the numbers observed by this philosopher are affected by a small error equal to the dilatation of the liquid portion contained in the tube of his thermometers at each of the temperatures at which he made an observation. On that account it would be interesting that an exact philosopher would repeat these experiments again, to give them all the precision of which they are capable.

After this remark I set out from the formulas which we have established, and I shall endeavour to deduce from them the true and absolute dilatations.

This is easily done. To regulate the thermometers, Deluc put them first in melting snow, and then in boiling water. He marked in each of these two cases the extremity of the liquid column, and he divided the interval between them into 80 equal parts. Of consequence, the apparent and absolute dilatation of the liquid employed being denoted by D, this dilatation determines the extent of the 80°. Hence knowing D_T, that is to say, the number of degrees of the same thermometer corresponding to the temperature T, we can easily deduce from that the apparent dilatation Δ_T; for we shall have proportionally

$$\Delta_T = \frac{D}{80} D_T$$

But if we call the true and absolute dilatation δ_T, by which is meant the dilatation that would be perceived in a vessel which does not itself dilate, we have seen that it may be calculated from the apparent dilatation, and that we have in general

$$\delta_T = KT + \{1 + KT\} \Delta_T$$

K being the cubic dilatation of the matter of the vessel, in which the apparent dilatation Δ_T is observed; therefore if we put here, instead of Δ_T, its value, a function of D, we obtain

$$\delta_T = K\,T + D\,\frac{\{1 + K\,T\}}{80}\,D_T$$

Finally, if we substitute for D_T the general expression $A\,T + B\,T^2 + C\,T^3$, which we have verified, we obtain

$$\delta_T = K\,T + D\,\frac{\{A\,T + B\,T^2 + C\,T^3\}\,\{1 + K\,T\}}{80}$$

or, by actual multiplication,

$$\delta_T = \{K + \frac{D\,A}{80}\,T + \frac{\{B + A\,K\}}{80}\,D\,T^2 + \frac{\{C + B\,K\}}{80}\,D\,T^3 + \frac{K\,C\,D}{80}\,T^4.$$

The term containing T^4 is always insensible, unless we suppose the experiments extremely exact. It would not give for water $\frac{32}{10000000}$ of the primitive volume, even supposing $T = 80$. Therefore, neglecting this term, when the total dilatation, D, for a liquid is known, we must substitute its value in this formula. Making

$$a = \frac{D\,A + K}{80};\ b = D\,\frac{\{B + A\,K\}}{80};\ c = D\,\frac{\{C + B\,K\}}{80}$$

we have for every other temperature the true and absolute dilatation δ_T by this general formula,

$$\delta_t = a\,T + b\,T^2 + c\,T^3$$

which is that which we announced in beginning this investigation; but if the experiments employed could be regarded as excessively precise, perhaps the term involving T^4 might become sensible. Then it would be necessary to introduce a term of the fourth order in calculating D_T from the observations. I must remark that, our degrees being expressed according to the thermometer of Reaumur, we must take likewise for K the cubic dilatation of the vessel for one of these degrees.

All the experiments of Deluc were made in glass thermometer tubes. According to the experiments of Lavoisier and Laplace, the cubic dilatation of this species of glass is 0·0000262716 for each degree of the centesimal thermometer. Therefore, if we multiply it by $\frac{10}{8}$, or add to it one fourth of the amount, we shall obtain the dilatation for each degree of Reaumur, which will be

$$K = 0{\cdot}00003284$$

Hence we have only to determine by experiment the total and apparent dilatation D. Unfortunately, we cannot say that there are any liquids the dilatations of which are known with that precision with which philosophers at present conduct their experiments.

In this uncertainty I shall endeavour at least to calculate the dilatations of water and alcohol from the experiments on these two liquids made by Blagden and Gilpin, introducing the dilatation of the vessel. These experiments, indeed, do not extend beyond 0 and 30·2 Reaumur; but as they were made with very great care, their precision may supply their want of extension. Besides, this will be a method of verifying our formulas, as we shall deduce them from values which philosophers may hereafter verify by direct experiments.

I shall begin with alcohol. By comparing the weights of the same volume of liquid observed by Gilpin and Blagden for 30°, 35°, and 40° Fahrenheit, I have deduced by interpolation the weight of the same volume for 32° which corresponds with 0 of our thermometer. Then comparing that result with the weights observed at 50°, 70°, 95°, and 100° of Fahrenheit, I deduced from them the volumes at these different temperatures, taking the volume at 32° for unity. Thus I obtained the following results:—

Degrees of the mercurial thermometer.	Volume of alcohol observed.	Dilatation from the heat of freezing water. δ_T
32° F or 0 R	1·000000	0·000000
50 ...... 8·00	1·010003	0·010003
70 ...... 16·89	1·021750	0·021750
95 ...... 28·00	1·037569	0·037569
100 ...... 30·22	1·040525	0·040525

To deduce from these results the total dilatation D from 0 to 80° R., I shall employ the last two observations, and I shall regard them as values of δ_T given. Then in the equation

$$\delta_T = K T + D \frac{\{A T + B T^2 + C T^3\}\{1 + K T\}}{80}$$

every thing is known except D. We may, therefore, deduce it from that equation. In the first place, by calculating D_T and K T we find

$T = 28·000$; $A T + B T^2 + C T^3 = 23·758$; $K T = 0·00091952$
$T = 32·222$; $A T + B T^2 + C T^3 = 25·808$; $K T = 0·00099248$

Then from observations we have

$$T = 28·000;\ \delta_T = 0·037369;\ \delta_T - KT = 0·036449;\ \frac{\delta_T - K T}{1 + K T} = 0·036416$$

$$T = 32·222;\ \delta_T = 0·040525;\ \delta_T - KT = 0·039533;\ \frac{\delta_T - K T}{1 + K T} = 0·039494$$

By substituting these values in the formula we obtain two equations;

$$0·036416 = \frac{23·758}{80} D;\quad 0·039494 = \frac{25·808}{80} D$$

The first of these equations gives

$$D = 0·122649;$$

The second gives

$$D = 0·122424$$

These only differ from each other $\frac{1}{10000}$ of the primitive volume at 0. I take the mean of them, and thus obtain

$$D = 0{\cdot}122536$$

This is the apparent dilatation of alcohol in glass from 0° to 80° R. To obtain the true dilatation, we must deduce it from the formula

$$\delta_T = KT + \{1 + KT\}\,\Delta_T$$

which, making $T = 80$, gives

$$\delta_{80} = 80\,K + \{1 + 80\,K\}\,D$$

Substituting for D its value obtained above, we have for strong alcohol

$$\delta_{80} = 0{\cdot}1254852$$

or very nearly $\frac{1}{8}$. This is the true dilatation from 0° to 80°. I am not acquainted with any other indication on this subject, except that of Nollet, who in his Lessons de Physique, tom. 4, p. 379, says that alcohol dilates 0·087 in passing from the freezing temperature to that of boiling water. In order to compare this result with ours, we must remark that Nollet observed the dilatation of the liquid in a tube of glass, at the extremity of which he had blown a ball, so that the value which he gives is the apparent dilatation, which approaches to the value of our D. There is an obvious reason why the dilatation should appear to him less than we have found it here. His tube was open, and his alcohol not freed from air. This would cause it to boil before it reached the temperature of boiling water; but as soon as it began to boil it would not become hotter, and of course would not dilate any further. Hence it never dilated so much as the alcohol did in Deluc's experiments, in which it was inclosed in a vessel hermetically sealed and freed from air. What fully confirms this consideration is, that if we calculate the value of D_T corresponding to the apparent dilatation $\Delta_T = 0{\cdot}087$, which may be done in this manner:—

$$D_T = \frac{80}{D}\,\Delta_T$$

we obtain

$$D_T = 56{\cdot}79$$

that is to say, that at this dilatation the alcohol thermometer void of air marks 56·79° on its own scale; which, according to the table of Deluc and our formula corresponds with 60·7° of the mercurial thermometer; and this is nearly the temperature at which alcohol boils in the open air.*

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* Since this paper was written, M. Berthollet has shown me the Philosophical Chemistry of Mr. Dalton. I there found an experiment made by that skilful philosopher which fully confirms the value given in the text of the absolute dilatation of alcohol. I shall give the calculation hereafter.

(To be continued.)

Article VII.

On Cubic Equations. By Mr. Horner.

(To Dr. Thomson.)

DEAR SIR, *Bath, Jan.* 17, 1817.

Having frequently, when reading or investigating subjects connected with cubic equations, experienced the advantage of having by me a compressed syllabus of the simple relations existing between the roots and the numeral parts, and conceiving that the same convenience will be acceptable to others, I beg to submit the following specimen through the medium of the *Annals of Philosophy.* To avoid the pedantry of unnecessary reference to authorities, I have indicated elementary sources of each transformation, in a connected series.

Let the proposed equation be

$$x^3 - bx - c = 0 \qquad (1)$$

Compare it with

$$(x - R) \times (x + r) \times (x + \varrho) =$$
$$x^3 - (R - r - \varrho)\,x^2 - (Rr + R\varrho - r\varrho)\,x - Rr\varrho = 0 \;\therefore \qquad (2)$$

This comparison gives us,

First, $R - r - \varrho = 0$ .... (3)

Or, $R = r + \varrho,\ r = R - \varrho,\ \varrho = R - r$ .... (4)

Secondly, $b = Rr + R\varrho - r\varrho$ .... (5)

Which, by substituting from equations 4, becomes

$b = R^2 - r\varrho$, or $Rr + \varrho^2$, or $R\varrho + r^2$ .... (6)

The sum of these, compared with eq. 5, gives

$b = \frac{1}{2}(R^2 + r^2 + \varrho^2)$ .... (7)

Comparing the square of (5) with (3),

$b = \sqrt{(R^2 r^2 + R^2 \varrho^2 + r^2 \varrho^2)}$ .... (8)

Comparing this with the square of (7),

$b = \sqrt{\dfrac{R^4 + r^4 + \varrho^4}{2}}$ .... (9)

Substitute (4) in (6), and we have

$b = R^2 - Rr + r^2$, or $R^2 - R\varrho + \varrho^2$, or $r^2 + r\varrho + \varrho^2$ .... (10)

which is equivalent to

$b = \dfrac{R^3 + r^3}{R + r}$, or $\dfrac{R^3 + \varrho^3}{R + \varrho}$, or $\dfrac{r^3 - \varrho^3}{r - \varrho}$ .... (11)

Substitute $R,\ -r,\ -\varrho$, for x in (1), and

$b = R^2 - \dfrac{c}{R}$, or $r^2 + \dfrac{c}{r}$, or $\varrho^2 + \dfrac{c}{\varrho}$ .... (12)

Thirdly, $c = Rr\varrho$ .... (13)

Or, on comparing with (4),

$$c = R^2 r - R r^2, \text{ or } R^2 \varrho - R \varrho^2, \text{ or } r^2 \varrho + r \varrho^2 \quad \ldots\ldots\ldots\ldots (14)$$

By equation (1) or (6) or (12),

$$c = R^3 - b R, \text{ or } - r^3 + b r, \text{ or } - \varrho^3 + b \varrho \quad \ldots\ldots\ldots\ldots (15)$$

Comparing the sum of these with (3),

$$c = \tfrac{1}{3} (R^3 - r^3 - \varrho^3) \quad \ldots\ldots\ldots\ldots (16)$$

The sum of equations (14) gives

$$c = \tfrac{1}{3}(R^2 r + R^2 \varrho - R r^2 - R \varrho^2 + r^2 \varrho + r \varrho^2) \quad \ldots\ldots (17)$$

Comparing (15) with (7)

$$c = \tfrac{1}{2} R (R^2 - r^2 - \varrho^2), \text{ or } \tfrac{1}{2} r (R^2 - r^2 + \varrho^2), \text{ or } \tfrac{1}{2} \varrho (R^2 + r^2 - \varrho^2) \,.. \quad (18)$$

For reasons, which your printer will by this time conjecture, I refrain from multiplying these beautiful and interesting analogies to the utmost. Still excluding binomial and more complicated functions of b and c, I shall close this table with a few of those in which the reciprocals of the roots are concerned.

Dividing (5) by (13),

$$\frac{b}{c} = -\frac{1}{R} + \frac{1}{r} + \frac{1}{\varrho} \quad \ldots\ldots\ldots\ldots (19)$$

Dividing the square of (8) by that of (13),

$$\frac{b^2}{c^2} = \frac{1}{R^2} + \frac{1}{r^2} + \frac{1}{\varrho^2} \quad \ldots\ldots\ldots\ldots (20)$$

Dividing eq. (3) by eq. (13),

$$\frac{1}{R r} + \frac{1}{R \varrho} - \frac{1}{r \varrho} = O \quad \ldots\ldots\ldots\ldots (21)$$

Dividing twice (7) by the square of (13),

$$\frac{2b}{c^2} = \frac{1}{R^2 r^2} + \frac{1}{R^2 \varrho^2} + \frac{1}{r^2 \varrho^2} \quad \ldots\ldots\ldots\ldots (22)$$

Dividing thrice (16) by the cube of (13),

$$\frac{3}{c^2} = -\frac{1}{R^3 r^3} - \frac{1}{R^3 \varrho^3} + \frac{1}{r^3 \varrho^3} \quad \ldots\ldots\ldots\ldots (23)$$

Dividing $(9)^2$ by $(13)^4$,

$$\frac{2b^2}{c^4} = \frac{1}{R^4 r^4} + \frac{1}{R^4 \varrho^4} + \frac{1}{r^4 \varrho^4} \quad \ldots\ldots\ldots\ldots (24)$$

Dividing eq. (14) by the square of (13),

$$\frac{R^2}{c} = \frac{1}{r} + \frac{1}{\varrho}, \quad \frac{r^2}{c} = \frac{1}{\varrho} - \frac{1}{R}, \quad \frac{\varrho^2}{c} = \frac{1}{r} - \frac{1}{R} \quad \ldots\ldots\ldots\ldots (25)$$

Dividing (17) by (13),

$$\frac{R}{r} + \frac{r}{R} + \frac{R}{\varrho} + \frac{\varrho}{R} - \frac{r}{\varrho} - \frac{\varrho}{r} = 3 \quad \ldots\ldots\ldots\ldots (26)$$

The reader who will take the trouble to refer to my papers of October and November last will perceive the facilities gained in that investigation by employing the 12th and 6th of the present series of equations. As a further exemplification, I shall here only notice the elementary case, in which one root, as R, being known, the general values of the other two roots are required. These are enveloped in the quadratic equation

$$(x + r) \times (x + \varrho) = x^2 + (r + \varrho) x + r \varrho = O \quad \ldots\ldots\ldots (\text{I.})$$

On comparing this with equations (4), (6), (13), in order to obtain a formula involving only x and known quantities, we have the option of two such formulæ, viz.

$$x^2 + \mathrm{R}\,x + (\mathrm{R}^2 - b) = 0 \ldots\ldots\ldots\ldots\ldots\ldots \text{(II.)}$$

$$\text{and } x^2 + \mathrm{R}\,x + \frac{c}{\mathrm{R}} = 0 \ldots\ldots\ldots\ldots\ldots\ldots \text{(III.)}$$

The solution of these equations gives us

$$x = -\tfrac{1}{2}\mathrm{R} \pm \tfrac{1}{2}\sqrt{4\,b - 3\,\mathrm{R}^2}$$

$$x = -\tfrac{1}{2}\mathrm{R} \pm \tfrac{1}{2}\sqrt{\mathrm{R}^2 - \frac{4\,c}{\mathrm{R}}}$$

Perhaps the most familiar way of considering the general relation of the roots is this: R the *greatest*, r *decreasing*, and ϱ *increasing*, in their progress from the nascent case, in which $r = \mathrm{R}$, and $\varrho = 0$. The correct interpretation of the solution just obtained will, therefore, be

$$\left.\begin{aligned} r &= \tfrac{1}{2}\,(\mathrm{R} \pm \sqrt{4\,b - 3\,\mathrm{R}^2}) \\ \varrho &= \tfrac{1}{2}\,(\mathrm{R} \mp \sqrt{4\,b - 3\,\mathrm{R}^2}) \end{aligned}\right\} \ldots\ldots\ldots \text{(IV)}$$

$$\text{Or,}\quad \left.\begin{aligned} r &= \tfrac{1}{2}\,\left(\mathrm{R} \pm \sqrt{\mathrm{R}^2 - \frac{4\,c}{\mathrm{R}}}\right) \\ \varrho &= \tfrac{1}{2}\,\left(\mathrm{R} \mp \sqrt{\mathrm{R}^2 - \frac{4\,c}{\mathrm{R}}}\right) \end{aligned}\right\} \ldots\ldots\ldots\ldots \text{(V.)}$$

the upper signs obtaining in the irreducible, and the under signs in the reducible case.

In equations 5, 8, and 7, 9, we have the solutions of two curious *diophantine* problems; the common condition of limitation in the results being given in equation 3.

The analogies traced in this paper being distinct from the general theory of cubic equations—on which, if agreeable, I propose to send you a memoir on a highly condensed but comprehensive plan—are offered in the form of a detached essay, as the most suitable to their character. Their utility, of which I have specified two instances only, will abundantly appear on applying them to other incidental cases, or to any particular form of a reduced cubic, such as

$$x^3 - 3\,p^2\,x - 2\,p^2\,q = 0,$$

used by Cotes in his Logometria; or

$$x^3 - d\,x - d = 0,$$

which Mr. Lockhart has so ingeniously employed in his method of approximation.

W. G. Horner.

P. S. Having still a vacant space, I am tempted to put in a word on the curvature of the circle, which has been so much agitated lately in the *Annals*. No mathematician certainly has ever regarded the circle as a polygon of any *finite* number of sides; all the ingenious remarks which have been founded on such a supposition are, therefore, quite irrelevant. Only contemplate the number of sides as infinite, and a simple consideration will establish the truth of the idea which has been combated. The versed sine which bisects an arc, and is the measure of its deviation from the state of lying "evenly between its extreme points," is a third proportional to the diameter, and the chord of half the arc. When this chord, then, becomes evanescent, or null in comparison of the diameter, the bisecting versed sine becomes also null in comparison of the arc. So that an evanescent or *infinitesimal arc is in geometrical strictness a right line.*

Deviation from rectilinearity is not exactly the sense in which the term curvature is to be understood in the fluxionary analysis; but it is the only interpretation the word can receive when applied to different portions of the same circle. On the principle just assumed, the curvature of any arc φ may then be estimated as $\left(\frac{\text{ver. sin. } \frac{1}{2}\,\varphi}{\text{sin. } \frac{1}{2}\,\varphi} =\right) \text{tan. } \frac{1}{4}\,\varphi$, the curvature of the semicircle being the unit of comparison.

If S_1, S_2, S_3, S_4, represent three successive sums of the powers of the roots of the equation $x^3 - b\,x - c = 0$, the theory of recurring series gives $S_4 = b\,S_2 + c\,S_1$. Hence the following uninomial functions of b and c, in addition to those in this paper:—

$$b\,c = \tfrac{1}{5}\,(R^5 - r^5 - \varrho^5) \;\ldots\ldots\ldots\ldots \quad (27)$$

$$b^2 c = \tfrac{1}{7}\,(R^7 - r^7 - \varrho^7) \;\ldots\ldots\ldots\ldots \quad (28)$$

$$\text{whence } b = \frac{5\,(R^7 - r^7 - \varrho^7)}{7\,(R^5 - r^5 - \varrho^5)} \;\ldots\ldots\ldots\ldots \quad (29)$$

ARTICLE VIII.

Queries respecting the Probability of reaching from the Island of Spitzbergen the North Pole, by Means of Rein-deer, during the Winter; and answered by Persons who wintered there. By Col. Beaufoy, F. R. S.

(To Dr. Thomson.)

MY DEAR SIR, *Bushey Heath, Feb.* 11, 1817.

Some years past I was impressed with the idea of the possibility of reaching the North Pole from Spitzbergen during the winter by travelling over the ice and snow in sledges drawn by rein-deer. Therefore, with the view of determining how far this plan was practicable, I sent several queries, and requested answers to them from Russians who were at that time living at Archangel, and had wintered in those remote islands. Those queries, together with the answers, I take the liberty of transmitting to you, as I learn from conversation that the practicability of such a journey conducted in a similar manner is entertained by well-informed persons; and, before a plan is put in execution, it is desirable to know what has been previously done on the same subject. If you should deem these queries worthy of a place in your *Annals*, I shall be flattered by their insertion. The 31st and 33d seem contradictory, probably from some error in translating the questions into Russ, or the answers into English.

I remain, my dear Sir, very sincerely yours,

MARK BEAUFOY.

1. *Query.*—How many settlements have the Russians on the Island of Spitzbergen, and which is the most northerly?

Answer.—There are neither settlements nor fixed inhabitants in Spitzerbergen, except those fishermen who go there in quest of fish, and likewise of those animals from Megen, Archangel, Onega, Rala, and other places bordering the White Sea, in vessels from 60 to 160 tons. They sail from the above-mentioned places, those for the summer fishery in the beginning of June, and those for the winter in June and July. They arrive on the west side of Spitzbergen, and commonly return home, the former some year in September, and the latter the next year in August and September. They winter in the Gulphs of Devil Bay, Clock Bay, Ring Bay, Crus Bay, German Island, Magdalene Bay, and to the northward in Liefde Bay, and others. The furthest north our fishermen ever have sailed to is Liefde Bay, and from thence in small boats as far as Nordoster Island.

2. *Q.*—At what time of the year does the winter commence?

A.—The winter generally sets in about the latter end of September and beginning of October.

3. *Q.*—Is it ushered in by storms? and is any one wind particularly productive of them?

A.—The winter sometimes sets in with winds from the N., N.N.W., and N.W.; and sometimes commences with calm weather, hard frosts accompanied with snow.

4. *Q.*—Is the weather generally speaking calm in winter, or are the winds high?

A.—The winds are very high and frequent; so that two-thirds of the winter may be said to be boisterous.

5. *Q.*—What quantity of snow do you suppose falls annually; that is, to what depth on the ground?

A.—On even places the snow is from three to five feet deep; but the winds drive it from place to place, so as sometimes to render all passage impracticable; and on the coats between the hills there are mountains of ice, occasioned by the pressure of the waters and drift of snow.

6. *Q.*—Are the storms of snow frequent, and of long duration?

A.—The storms of snow are very frequent, continuing for two, three, and four days, and sometimes for as many weeks; but the latter do not occur above once or twice in a year.

7. *Q.*—Is the cold much more severe at Spitzbergen than at Archangel? Has the degree ever been ascertained by the thermometer? If it has, what was it?

A.—From the fishermen's remarks, the cold is more severe at Spitzbergen than at Archangel; but the degree is not known, as the people who go there have no thermometers.

8. *Q.*—Is the cold ever so intense as to render going abroad dangerous?

A.—The cold is never so severe as to hinder the fishermen, they being accustomed to it, from exposing themselves; but sometimes the winds and drifts of snow confine them to their huts.

9. *Q.*—Admitting it to be so, by what exercise do the Russians keep off the scurvy?

A.—When the last-mentioned weather is an obstacle to their leaving their huts, they keep off the scurvy by the exercise of throwing the snow from off and around their huts, which from stormy weather are often buried; and in order to get out, they are then obliged to make a passage through the roof. They likewise oppose the distemper by making use of a particular sallad or herb, which grows there on stones, and with which they generally provide themselves in due time against winter; but sometimes, from necessity, they are obliged to dig through the snow for it. Some of it they eat without any preparation; and a part they scald with water, and drink the liquid. They also carry with them for the same purpose, as a preventive, a raspberry, called in Russia *moroshka*, which they preserve by baking with rye flour, which they eat; and when pressed, drink the juice. They also take fir tops with them, which they boil; and the water they drink as an antidote likewise against the scurvy.

10. *Q.*—In what manner are the huts constructed?

A.—The huts the people use they always take with them in their vessels, and on their arrival there put them together. They are constructed of thin boards, and in the same manner as the peasants' houses here. They likewise generally take bricks with them for building their stoves; but when they fall short, clay found there is made use of in their stead. Their largest hut, which is erected in the neighbourhood of their vessels, boats, &c. is from 20 to 25 feet square, and serves as a station and magazine; but those huts the men erect who go in quest of skins are only from seven to eight feet square, and in the autumn are carried along the shores in boats, and put up at distances from each other of 10 to 50 Russian versts. They take the necessary provisions with them for the whole winter to serve two or three men, as many generally occupying each hut.

11. *Q.*—What fuel have they, and in what manner are their huts heated?

A.—The fuel commonly used for heating their huts is wood, which they likewise bring with them in their vessels, and land at the station hut. In autumn the necessary quantity for heating the aforesaid small huts is conveyed in boats, or on small hand sledges, to the destined places. They often meet with wood there too, thrown by the sea on the shores.

12. *Q.*—On what kinds of provisions do the Russians subsist during the winter?

A.—The provisions they subsist on during the winter consist in rye flour (of which they make bread), salt beef, salt cod, and salted holybut, butter, oat and barley meal, curdled milk, peas, honey, linseed oil; all which they bring to Spitzbergen with them, and divide the same proportionally by weight to each man. Their employers allow them provisions for one year and a half, besides which the fishermen kill wild lion deer in winter, and birds in summer, which are experienced to be excellent food, and very healthy.

13. *Q.*—Do they chiefly use spirituous or malt liquors?

A.—They chiefly drink a liquor called *quas*, made from rye flour and water. Malt and spirituous liquors are entirely excluded and forbidden by their employers, to prevent drunkenness, as the Russians, when they had it, drank so immoderately that work was often neglected entirely.

14. *Q.*—When in the open air, how do they defend themselves?

A.—They defend themselves from the rigour of the weather by a covering made of skin, above which they wear another made of the skin of rein deer, called *kushy*, and wear boots of the same.

15. *Q.*—Do they not use masks, and omit the practice of shaving?

A.—They use no masks, nor do they shave; but they wear a large warm cap, called *truechy*, which covers the whole head and neck, and most part of the face. They also wear gloves of sheepskin.

16. *Q.*—Do the inhabitants cross the country during the winter?

A.—There are no inhabitants, as said before; but the fishermen who are there for a time do go over from one island to the other of small distances?

17. *Q.*—How do they travel, at what rate, and how carry the necessary stock of provisions for their subsistence during the journey?

A.—They travel on foot; that is, on snow skaits, and draw their food after them in small hand sledges; but those who bring dogs with them make use of the same. When travelling, snow is their drink. Horses or rein-deer would be of no use to them for the conveyance for their provisions; nor have they any.

18. *Q.*—By what means do they procure water; and is it by melting snow, or do they find springs?

A.—They use spring water when it is to be had, often take it from lakes, and from necessity sometimes dissolve snow; but it seldom happens that they are in want of fresh water, because they commonly pitch on those places where it is to be met with.

19. *Q.*—Is not the ice so firmly consolidated as to render all passage across it from one island to the other perfectly safe during winter?

A.—The ice at Spitzbergen is well consolidated; and in some places the flakes run to a great height, one on another, which makes even the passage on foot very difficult; other places are quite smooth, except those gulfs which run in the land to about 20 versts, where the ice is continually floating and drifting; but travelling with horses or rein-deer is quite impossible.

20. *Q.*—Is not the ice rendered smooth by the interstices being filled up with snow?

A.—As before said, the ice is made smooth by the snow filling up the inequalities.

21. *Q.*—Does any danger arise either in crossing the land or the ice, from the drifting of the snow?

A.—They do not journey in winter, as before mentioned, except to islands at trifling distances; and a traveller is in much danger if surprised by a sudden gale of wind, accompanied by drifts of snow; he is obliged to lie down, covering himself with his ———, and remain so secured till the hurricane is over; but when it continues for any length of time, the poor wretch often perishes.

22. *Q.*—What degree of light is there in winter?

A.—The fishermen do not know what the *degree* of light may be in winter; indeed, they are ignorant of the meaning of the term: however, they say from the latter end of October to the 12th of January the sun does not appear above the horizon, which causes a continual darkness, and obliges them always to keep a light in their huts by burning train oil in lamps; but as soon as the sun makes its appearance, the days increase very rapidly.

23. *Q.*—What difference does the absence of the moon occasion? Are the stars in general brilliant? Can you see to read when the moon is under the horizon?

A.—From the appearance of the moon in her second quarter to her decline in the last, the nights are very luminous, and the stars extraordinarily light both day and night. In the gloom of winter the people keep time from the position of certain stars. When the moon is below the horizon, it is impossible to read.

24. *Q.*—Is the Aurora Borealis very brilliant; and in what part of the horizon is it seen?

A.—In the dark time of winter the Aurora Borealis is commonly seen most strong in the N., and appears very red and fiery.

25. *Q.*—Does it appear possible to cross the ice in winter to the North Pole? If it does not, what are the obstacles?

A.—The likelihood of a passage to the North Pole does not seem probable to the fishermen, as they have not had an opportunity to attempt it; and, from their observations, think all passage impossible, as the mountains of ice appear monstrously large and lofty. Some of the ice is continually drifting about; so that in many places water is discerned. Those who have been on the most elevated parts of Nordester Island declare that, as far as it is visible, open water is only seen; but to what distance it may continue so, it is impossible for them to ascertain, as an attempt for the discovery has never been made; but seemingly it is practicable to bring the fuel and provisions in vessels to the Nordester Island.

26. *Q.*—If the passage should be deemed practicable, in what manner should it be attempted? and what means of conveying fuel and provisions appear to be the best?

A.—As the fishermen think all passage impracticable, it is not in their power to give any answer to this demand.

27. *Q.*—Might not three different huts constructed like those in which the people of Spitzbergen live, together with a sufficient quantity of provisions in each for half a dozen of people, be conveyed on sledges, and be left at the different distances of 200, of 400, of 600 miles, N. of Spitzbergen, as places of deposit for the assistance of those who shall undertake the journey?

A.—Such huts might be built, and placed on shore, as said in the tenth article, at a convenient distance from their vessels; but as for conveying them ready-built to the distances proposed appears to the people an impossibility.

28. *Q.*—What number of persons and rein-deer, or of dogs, would be requisite for conveying the huts?

A.—From the mountains of ice and great falls of snow, neither dogs nor rein-deer would be able to draw loads; for the fishermen themselves, to be as light as possible, go on snow skaits.

29. *Q.*—At what price per man for each day's journey would the people of Spitzbergen, if they think the adventure practicable, be likely to undertake the conduct of the sledges?

A.—As in the last reply the fishermen show it is not convenient there to draw with dogs or rein-deer, therefore no price can be said.

30. *Q.*—Are there any persons in Archangel who have formerly resided in Spitzbergen who would engage in the business? and are there any who would be willing, in company with two Englishmen, to attempt on this plan a passage to the North Pole?

A.—As there are not, nor ever were, any natives of Spitzbergen, none therefore can be resident in Archangel: however, many men may be met with here who have wintered there; but as they have never made an attempt to go to the Pole, they cannot undertake the conduct of the business. Notwithstanding, if an Englishman should determine on the endeavour, some people might be met with who would perhaps, with an English ship's company, engage themselves.

31. *Q.*—In the spring, have flights of birds ever been observed to direct their course N. of Spitzbergen?

A.—It has been always experienced by those who have been at the most northerly parts of Spitzbergen that in the spring a great number of wild geese, ducks, and other birds, take their flight further north.

32. *Q.*—What animals and birds have they during the summer, and what species winter on the island?

A.—In Spitzbergen they have wild rein-deer, white and blue foxes, and white bears, which remain continually on the island; but geese, ducks, &c. are only there in summer.

33. *Q.*—Those which quit Spitzbergen on the approach of winter, in what month do they generally emigrate, and to what point of the compass?

A.—All the before-mentioned birds on the approach of winter, that is, in the latter end of September, fly to the southward, and return again in the latter end of April.

N.B. The 31st and 33d answers do not apparently agree.

ARTICLE IX.

On the Temperature at which Water is of the greatest Density.

By George Oswald Sym, M.A.

It might be thought a very simple problem to determine at what temperature a given quantity of water occupies the smallest space; yet, though a greater share of attention and ingenuity has been bestowed upon this subject than its importance may seem to merit, no certainty with regard to it has hitherto been attained. Most chemists believe the greatest density of water to be at or near 40°; but some of very high authority place it at 36°; and there are probably not a few who still indulge a philosophical reluctance to admit that the condensation can be any where greater than at the freezing point. The question is, therefore, still undecided, and continues to afford a proof how difficult it is to establish a fact of which no good explanation can be given.

In addition to the various methods which have been contrived for settling this point, I venture to propose another, depending on the construction of an instrument, which seems adapted for directly measuring the real expansions and contractions of fluids.

In order to construct this instrument, two glass tubes are to be procured, equal to each other in length, and equal in weight; but so unequal in width, that the one may slide easily into the bore of the other, and leave a sensible space unoccupied all around. A certain portion at the end of the larger of these tubes is to be melted, and blown into a bulb. An equal portion at the end of the smaller is also to be melted, and so pushed back and compressed that the whole length of the two may still remain equal, and that the one may still be capable of sliding into the other. This done, the larger is to be nearly filled with distilled water; the smaller is then to be let down into it in its whole length, so that its thickened end may rest on the bottom of the bulb; and its other end is to be fixed in the middle of the bore, by wedging in around it some small slips of leather or of cork. Lastly, a scale is to be affixed to the neck, extending to some distance above and below the level at which the surface of the water now stands: and then the instrument will be fit for use.

Two tubes which consist of equal quantities of glass will be equally expanded or contracted by the addition or abstraction of equal quantities of heat. It is evident, also, from the construction of the instrument, that that portion of the inner tube which is contained within the neck of the outer consists of just as much glass as goes to constitute the neck itself; and that portion which is contained within the bulb, of just as much glass as goes to constitute the bulb itself. Hence any variation of temperature which either extends to the whole instrument, or, in equal proportions, to its corresponding parts, will occasion an equal variation of bulk, either in its two members, in general, or in the corresponding portions of them in particular.

But the space between these two members—that space in which the water is contained—will be oppositely affected by similar variations of bulk in the two members. Every expansion of the outer tube and bulb will tend to enlarge that space, and consequently to lower the level of the water in the neck; whereas every expansion of the inner tube will tend to diminish the same space, and to raise the level of the water: and the converse will obviously hold good with regard to contractions. Hence if the two members of the instrument be equally and simultaneously expanded or contracted, the space between them will neither be enlarged nor diminished. That space, therefore, can suffer no variation of capacity from any change of temperature which extends to the whole instrument, or in equal proportions to the corresponding parts of it; the level of the water in the neck can be affected by no such cause; and consequently, if it be raised or depressed, this must be occasioned by an actual change of density in the water itself. In short, we here call into exercise such a principle of compensation, that the effects of temperature on the glass will be completely neutralized, and the apparent changes in the volume of the water identified with the real ones.

Such, at least, would be the case if the instrument could be constructed with theoretrical perfection. But it must be confessed that there are some practical difficulties and sources of inaccuracy which cannot wholly be avoided. It is only by a rare chance that one can expect to light upon two tubes of different widths, which are, length for length, exactly of the same weight. Even supposing such tubes to be obtained, if the bulb be made very large, this renders it necessary to convert a considerable portion of the inner tube into an almost solid lump, in which form glass does not expand or contract quite so much as in an attenuated form. Or if, to avoid this necessity, the bulb be made comparatively small, the risings and depressions of the water in the neck will be proportionally minute and indistinct.

Yet these difficulties may, I think, be so far overcome as to remove any reasonable distrust of the general accuracy of the results. I procured two glass tubes of the requisite dimensions, equal lengths of which did not differ in weight so much as 5 gr. in 300; and I had the bulb blown of such moderate size that the inner tube did not require to be any where converted into a solid rod, and yet of such sufficient size that the expansions and contractions of the water were perceptible for one degree of temperature, and quite unequivocal for two degrees. I was, therefore, satisfied that the instrument was capable of determining, not only the existence, but very nearly the law, of the anomaly in question.

The result of my trials, which I repeated oftener and more scrupulously than an older experimenter would have thought at all necessary, is conformable to the generally received opinion. Whether I plunged the bulb into water at 50°, and then slowly cooled it down to 32°, maintaining the temperature for some time stationary at each successive interval of two or three degrees, in order to make sure of its being uniformly diffused throughout both members of the instrument; or whether I reversed the process, by first surrounding the bulb with ice, and then slowly communicating heat to it; I found that the condensation was greatest either at 40°, or in its near neighbourhood, and that the rate of expansion on each side of that limit was apparently the same. I even made the experiment with a greater degree of nicety than can be done with a graduated scale, by tying a fine hair round the tube, and placing it precisely opposite to the level of the water, after having kept it at 40° for more than half an hour; and I still found that any change of temperature, whether a rise or a fall, caused the water to ascend above the level of the hair. I am, therefore, almost convinced that the greatest density of water is at a temperature not differing from 40° by more than one degree; though I should not like to speak confidently on the subject, unless I could have the satisfaction of seeing the method approved, and the result verified, by some more experienced chemist.

Article X.

Magnetical Observations. By Col. Beaufoy, F.R.S.

(To Dr. Thomson.)

MY DEAR SIR, *Bushey Heath, April* 18, 1817.

Allow me to mention, these observations were made with the same instrument and needles as in the years 1813, 14, and 15; but the base of the instrument, instead of resting on a piece of mahogany, is now placed on a brass triangular stand, through the angles of which are inserted the three mill-headed screws for levelling it. By this improvement the instrument, when once levelled, retains its horizontal position, the wooden stand being liable to warp. The object glass also can now be adjusted for different distances. One needle is a very slender parallelopipedon, and weighs 51 gr.; the other is of a cylindrical form, terminating at each extremity with a cone, and weighs 63 gr. The increase of weight in the needles is owing to the new agates which have been put in.

In the subjoined meteorological table Fahrenheit's thermometer is used, and M. de Luc's hygrometer. The velocity of the wind is determined by taking the mean of the times it blew three different miles, and then reducing that mean to feet per second. The direction of the wind is counted from the true points of the compass.

I remain, my dear Sir, yours very sincerely,

Mark Beaufoy.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 42″ North. Longitude west in time, 1′ 20·7″.

Magnetical Observations, 1817. — *Variation West.*

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
April 1	8^h	45′	24°	33′	37″	1^h	45′	24°	43′	14″	6^h	20′	24°	33′	54″
2	8	42	24	32	25	1	45	24	47	09	6	20	24	37	08
3	8	48	24	31	06	1	49	24	50	02	6	18	24	37	20
4	8	49	24	32	01	1	50	24	48	32	6	20	24	41	16
5	8	47	24	31	41	1	50	24	49	08	6	45	24	36	44
6	8	45	24	35	39	1	45	24	46	53	6	40	24	38	04
7	8	50	24	33	53	1	55	24	46	29	6	40	24	37	06
8	8	45	24	35	45	1	40	24	44	50	6	40	24	36	48
9	8	45	24	37	25	1	45	24	42	00	6	30	24	33	43
10	8	40	24	32	23	1	40	24	44	55	6	40	24	35	25
11	8	40	24	31	06	1	50	24	43	28	—	—	—	—	—
12	8	45	24	30	07	1	45	24	44	02	6	45	24	35	39
13	8	35	24	30	11	1	40	24	45	01	6	45	24	35	40
14	8	45	24	30	19	1	50	24	45	46	6	50	24	35	14
15	8	45	24	30	55	1	45	24	45	49	6	45	24	34	36
16	8	45	24	29	03	1	40	24	44	30	6	45	24	35	34
17	8	45	24	29	36	1	45	24	43	38	6	45	24	35	43

Meteorological Table.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.
		Inches.				Feet.	
April 1	Morn........	30·142	46°	55°	SSW		Fine
	Noon........	30·123	53	44	S	8·456	Fine
	Even	30·058	52	43	ESE		Fine
2	Morn........	29·992	46	54	ESE		Fine
	Noon........	29·945	51	39	ENE	10·717	Fine
	Even	29·918	48	43	E		Fine
3	Morn........	29·905	48	53	ENE		Fine
	Noon........	29·902	57	37	E	17·895	Fine
	Even	29·905	50	40	E by N		Fine
4	Morn........	29·955	46	62	ENE		Fine
	Noon........	29·955	54	48	ENE	15·000	Fine
	Even	29·955	46	50	ENE		Fine
5	Morn........	29·921	40	85	NE		Foggy
	Noon........	29·921	49	61	NE	7·812	Fine
	Even	29·925	47	50	NE		Fine
6	Morn........	29·941	41	65	NNE		Fine
	Noon........	29·972	44	68	NNE	35·625	Cloudy
	Even........	30·000	40	59	NE		Cloudy
7	Morn........	30·058	41	66	ENE		Cloudy
	Noon........	30·020	48	48	E	15·672	Fine
	Even	30·000	42	50	E		Fine
8	Morn........	29·781	40	67	W		Foggy
	Noon........	29·652	55	47	SW	15·39	Fine
	Even	29·548	47	47	W by S		Fine
9	Morn........	29·530	42	61	NNE		Drizzle
	Noon........	29·580	44	47	NNE	6·976	Cloudy
	Even	29·595	41	46	NNE		Cloudy
10	Morn........	29·587	36	46	N W by N		Clear
	Noon........	29·620	38	39	NNW	18·191	Cloudy
	Even	29·700	34	38	NNE		Clear
11	Morn........	29·853	35	53	N		Clear
	Noon........	29·830	40	37	NNW	7·35	Clear
	Even	—	—	—	—		—
12	Morn........	29·630	40	65	WNW		Cloudy
	Noon........	29·582	45	60	NW	12·976	Cloudy
	Even........	29·582	44	61	NW		Fine
13	Morn........	29·655	45	78	NW		Drizzle
	Noon........	29·681	50	66	NNE	7·099	Cloudy
	Even........	29·680	50	54	NW		Fine
14	Morn........	29·641	47	68	N W by W		Cloudy
	Noon........	29·641	57	54	N	7·695	Fine
	Even........	29·641	51	61	N W by W		Fine
15	Morn........	29·555	51	72	NW		Cloudy
	Noon........	29·565	59	42	WNW	22·97	Clear
	Even	29·490	50	46	NW		Fine
16	Morn........	29·275	48	53	WNW		Showery
	Noon........	29·447	49	40	NNW	26·812	Fine
	Even	29·580	42	42	N by W		Fine
17	Morn........	29·732	39	51	N W by N		Cloudy
	Noon........	29·765	40	48	N	13·865	Cloudy
	Even	29·800	39	50	NNE		Cloudy

The term Clear means free from clouds.

ARTICLE XI.

ANALYSES OF BOOKS.

1. *Système des Animaux sans Vertèbres.* Paris, 1801.
2. *Extrait du Cours de Zoologie, &c.* Paris, 1812.
3. *Histoire Naturelle des Animaux sans Vertèbres.* Paris, 1815-16.

The above works were all written by De Lamark, who, in the first, divided animals into, 1. Those with vertebræ. 2. Those without vertebræ.

In the second work he has arranged animals into—

I. AVERTEBROSA.

* APATHIQUES. Class 1. *Infusoria.* 2. *Polypi.* 3. *Radiata.* 4. *Vermes* (*Epizoariæ ?*).

** SENSIBLES. Class 5. *Insecta.* 6. *Arachnides.* 7. *Crustacea.* 8. *Annelides.* 9. *Cirrhipedes.* 10. *Mollusca.*

II. VERTEBROSA.

*** INTELLIGENS. Class 11. *Pisces.* 12. *Reptilia.* 13. *Aves.* 14. *Mammalla.*

In his last work he proposed to have followed this arrangement; but in the supplement to the first volume (which is principally occupied with a second edition of his Philosophie Zoologique in the form of an introduction) he has thus arranged animals, with two additional classes:—

	Inarticulés.	Articulés.
APATHIQUES ...	Infusoria. Polypi. Tunicata. Radiata.	Epizoariæ. Vermes.
SENSIBLES	Acephala. Mollusca.	Insecta. Annelides. Arachnides. Crustacea. Cirrhipedes.
INTELLIGENS ..	Pisces. Reptilia. Aves. Mammalia.	

Class 1. INFUSORIA. Contains a part of the animals included under that name by Müller, and is divided into—Order 1. *Nuda,* without appendices. 2. *Appendiculata,* with appendages.

Class 2. POLYPI. Order 1. *Ciliati;* gen. Cercula, &c. 2. *Denudati;* gen. Hydra, &c. 3. *Vaginati;* sertularia, cellaria madrepora, isis corallina, spongia. 4. *Tubiferi;* lobularia, &c. 5. *Natantes;* pennatula, &c.

Class 3. RADIATA. Order 1. *Mollia;* beröe, medusa, &c. 2. *Echinodermata;* asterias, echinus, &c.

Class 4. *Tunicata;* contains the ascidiæ of Savigny, of which we gave a short notice in our number for March.

Class 5. *Vermes.* Order 1. *Nuda;* hydatis, tænia, monostoma, &c. 2. *Rigida;* echinorhynchus, trichiuris, gordius, &c. 3. *Hispida;* nais, &c.

Class 6. EPIZOARIÆ. Lernæa, &c.

Class 7. INSECTA. A. Mouth with a sucker. Order 1. *Aptera;* pulex. 2. *Diptera;* musca, &c. 3. *Hemiptera;* cimex, &c. 4. *Lepidoptera;* papilio, &c. — B. Mouth with mandibles. Order 5. *Hymenoptera;* apis, tenthredo, &c. 6. *Neuroptera;* libellula, ephemera, hemerobius, &c. 7. *Orthoptera;* gryllus, blatta, forficula, &c. 8. *Coleoptera;* scarabæus, &c.

Class 8. ARACHNIDES. Order 1. *Antennifera;* pediculus, podula, julus, scolopendra, &c. 2. *Palpifera;* nymphon, caris, ixodes, oribata, hydrachna, siro, scorpio, aranea, &c.

Class 9. CRUSTACEA. Order 1. *Cryptobranchia;* cancer, matuta, maïa, pagurus, galatea, &c. 2. *Gymnobranchia;* squilla, gammarus, ligia, caligus, cypris, &c.

Class 10. ANNELIDES. Order 1. *Cryptobranchia;* hirudo, lumbricus. 2. *Gymnobranchia;* arenicola, amphitrite, dentalium, &c.

Class 11. CIRRHIPEDES.* Balanus, &c.

Class 12. MOLLUSCA. Clio, doris, helix, sepia, carinaria, &c.

Class 13. ACEPHALA. Ostrea, Venus, &c.

ARTICLE XII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, March 27, Mr. Marshall's paper on the laurus cinnamomum was continued. He described the way in which the cinnamon was collected, the frauds practised by those employed in gathering it, and the way in which it is stowed in the ships to be transported to Europe. It is usually stowed along with black pepper, in order to save room; or if pepper be wanting, coffee is substituted in its place. The Dutch sometimes ordered an oil to be extracted from the coarser kinds of cinnamon which were not considered as fit for the home market. The method is simple. The bark is reduced to a coarse powder, macerated for some days in sea water, and then put along with water into a still. The oil comes over with the water. There are two kinds of oil obtained: a light oil which swims on the surface of the water, and a heavy oil which sinks to the bottom. The whole of the light oil separates in 24 hours; but the heavy oil continues to subside for ten or twelve days. 80 lb. of fresh bark yield 2¼ oz. of the light oil, and 5¼ oz. of the heavy. The product is a little diminished when the bark has been kept for some years before it is distilled.

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* Correctly Cirripedes.

Cinnamon when first separated from the branch has an orange colour, and a very agreeable fragrant odour. The colour diminishes and the smell nearly disappears by keeping.

Cinnamon is confined to the torrid zone. Besides Ceylon, it grows on the Malabar coast, in Cochin China, in Sumatra, Borneo, Celebes, the Isle of France, Guiana, Jamaica, and other West India islands.

On Thursday, April 17, the remainder of Mr. Marshall's paper was read. It was taken up with endeavouring to trace the origin of the terms cinnamon and cassia. Herodotus informs us that the Greeks adopted their term cinnamon from the Phenicians. The Phenicians probably would adopt the word used in India. The Malays express cinnamon by the phrase *kayu menes*, sweet wood; and Mr. Marshall is of opinion that this is the origin both of the words cinnamon and cassia.

At the same meeting a note by Mr. Thomas Knight was read. On looking over Mr. Spence's book on Logarithmic Transcendents he found the very same demonstration of the binomial theorem which he himself had lately presented to the Royal Society. The paper contained some observations on Mr. Spence's demonstration of a nature not to be read.

At the same meeting, a paper by Mr. Babbage on the Utility of Analogical Reasoning in Mathematics was announced, but was not of a nature to be read.

At the same meeting, a description of an Increaser of Electricity by Mr. Uppington was begun. He had invented the instrument in 1810, had found it useful in his private experiments, and had given an account of it to the late Lord Stanhope, which his Lordship had approved of. The present description consisted of extracts from Mr. Uppington's letters to Lord Stanhope.

On Thursday, April 24, Mr. Uppington's paper was concluded. As it consisted entirely of the description of an instrument, and referred to figures which I had not the means of seeing, it is not in my power to convey an intelligible account of it to my readers.

LINNÆAN SOCIETY.

On Tuesday, April 1, part of a paper by M. de Brisson was read, giving an account of hymenopterous and dypterous insects not yet described by systematic writers.

On Tuesday, April 15, a short account of an uncommon species of serpent found in Dorsetshire, and long ago described by Linnæus, was given by Mr. Rackett. It is more poisonous than the common viper.

At the same meeting a paper by Mr. Colebrook was read, describing some little known Indian plants.

GEOLOGICAL SOCIETY.

June 21.—A paper by Dr. Clarke on the Composition of a dark bituminous Lime-stone from the Parish of Whiteford, in Flintshire, was read.

The lime-stone in question is remarkable for making with the usual ingredients an excellent water cement. It appears from Dr. Clarke's analysis to consist of about 90 per cent. of carbonate of lime, the remainder being chiefly alumine, with minute portions of silex and bitumen.

A letter from Robert Anstie, Esq. of Bridgewater, was read.

This letter, with illustrative drawings, describes some fossil vertebræ, ribs, and scapula, of a large animal, probably of the genus lacuta, which have lately been discovered imbedded in lias limestone, near Kingsdon, between Somerton and Ilchester. It also describes a fossil fish, apparently of the genus clupea, which was found imbedded in lyas at East Quantock Head, in the Bristol Channel.

A paper on Magnesian Lime-stone by Hen. Warburton, Esq. V.P.G.S. was read.

The largest continuous deposit of magnesian lime-stone extends from Sunderland to the vicinity of Nottingham, where it suddenly terminates. It is disposed in horizontal beds lying conformably with the red marl by which it is generally covered, and with which it sometimes alternates. It is represented as covering part of the coal measures; but whether it lies conformable with these latter has not been ascertained.

The red marl is widely distributed along the tract of country which lies between Lancashire and the southern coast of Devonshire, lying horizontally, as in the North of England, on the inclined beds of the coal measures, and bounding them at their basset. In this red marl, beds of a breccia, the cement of which is magnesian carbonate of lime, have been observed by Dr. Bright at Kingswood, near Bristol; by Dr. Wollaston and Mr. Greenough at Cowbridge, in South Wales; and by Mr. Aikin at Caerdeston and Loton, in Shropshire. Insulated blocks of a similar rock have been observed by Mr. Warburton and the late Mr. Tennant incumbent on the lime-stone on the Mendip Hills, near Cheddar.

The relation between the red marl and the coal measures it is of great importance to have thoroughly ascertained. If the former be considered as one of the complete series of beds which succeed each other in an invariable order, then we might expect, by sinking through the red marl in any place, to arrive at coal; but if, on the contrary, we suppose any causes of partial destruction and denudation to have been in action immediately previous to the deposition of the red marl, then this rock may occur superincumbent on any other rock from granite to coal, and of consequence be no certain indication of the latter.

Nov. 1.—An extract from a letter addressed to the Secretary from Dr. Trail, of Liverpool, was read.

In addition to a former notice respecting the occurrence of magnetic iron and iserine in Cheshire, Dr. T. now states that, in consequence of the heavy rains of the last summer, he has been enabled to trace these minerals for several miles along the Cheshire shore of the Mersey. They are washed out of a bed of slightly cohering sand of inconsiderable thickness, which is covered by a thick bed of clay, and appears to extend through a considerable part of the hundred of Wirrall, the district which lies between the estuaries of the Dee and the Mersey.

A letter from Captain Marryat was read, in which he gives some account of the country in the immediate vicinity of Nice, on the north-west coast of Italy. The maritime Alps consist of calcareous mountains, which diminish in height as they approach the coast, and finally sink into the Mediterranean Sea. The rock on which the citadel of Nice is built is a part of this extensive formation, being composed of a compact greyish marble, with white and yellow streaks. The upper part of the rock, and generally speaking of the whole of this formation, at least in the vicinity of the bay of Nice, is very much shattered and dislocated, and the rents thus occasioned are filled up by a hard red breccia, containing broken bones, together with land, river, and sea shells. These fossil remains are perfectly similar to those which occur in analogous situations in the rock of Gibraltar, and elsewhere on the coast of the Mediterranean, and appear to bear a great resemblance to those described by Cuvier, which are found in the neighbourhood of Paris. Near Villa Franca this breccia may be observed passing into a bed of clay, with fragments of lime-stone, which rests upon the compact calcareous rock; and on the heights of Cimiers the same breccia is covered by a bed of reddish gypsum. The peninsula of St. Hospice is formed of the compact calcareous rock already mentioned; but on the neck connecting this with the main land, and at an elevation about 22 yards above the present level of the sea, is a deposit of considerable thickness, and of much more recent origin. From the external appearance of this, and from the evidence obtained by sinking a well in it to a considerable depth, it appears to be a mixture of sand and of shells without any distinction of beds or of structure, except that in some cases the ingredients are compacted by a calcareous infiltration into a porous mass, while in other parts they are quite loose. The shells, of which a copious list is given, are stated by Capt. M. to be of the same species with those at present inhabiting the adjacent shores; and, what is remarkable, are also for the most part the same with those contained in the beds above the chalk in the basins of Paris and of the Isle of Wight.

The reading of a paper by Dr. Berger, entitled, Geognostic Remarks on the Rocks in the immediate Vicinity of Dublin, was begun.

ARTICLE XIII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Mr. Bakewell will deliver a series of Lessons in Geology at the Argyle Rooms, to commence the middle of the present month (May). The mode of instruction by lessons in a science presenting new and interesting objects of inquiry at every step will be found to possess many advantages, admitting questions and explanations which are precluded by the formality of public lectures.

Dr. Clutterbuck will begin his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Monday, June 2, at ten o'clock in the morning, at his house, No. 1, in the Crescent, New Bridge-street, Blackfriars.

II. *Register of the Weather at New Malton.*

January.—Mean pressure of barometer, 29·584; max. 30·61; min. 28·10. Range, 2·51 inches. Spaces described, 12·69 inches. Number of changes, 22.—Mean temperature, 36·80°; max. 54°; min. 24°. Range, 30°.—Amount of rain and snow, 1·31 inch. Wet days, 10.—Prevailing winds, S. and S.W. S.S.W. 11; W. 5; S.W. 11; N.W. 3; Var. 1.—Mean of the hygrometer at nine, a. m. 74½ nearly.

The character of this period during the former part was windy and changeable, with slight showers at intervals. From the 7th to the 23d the weather was cold and wet, with frequent showers of snow; and on the 16th it fell in considerable quantity, followed by rain, and a violent gale from the south. On the 23d a rapid increase of temperature and pressure took place, and the weather to the close was as calm and mild in these parts as it often is at the beginning of May.

The barometer has again been very low. The minimum occurred on the 20th, during a heavy storm of wind and rain from the S. For six preceding days the wind had blown steadily from that quarter, and the column was uniformly under 29·00; but on the 31st it indicated its maximum of elevation.

The variations towards the beginning of the month were again considerable, and the column throughout was in continual fluctuation.

February.—Mean pressure of barometer, 29·658; max. 30·61; min. 28·90. Range, 1·71. Spaces described, 13·32 inches. Number of changes, 24.—Mean temperature, 41·82; max. 53; min. 31. Range, 22°.—Amount of snow and rain, 1·34 inch. Total this year, 2·65 inches.—Wet days, 6; cloudy, 13; fine, 9. —Prevailing winds, W. and S.W. N. 2; S. 2; S.W. 6; W. 15; N.W. 2; Var. 1. Number of windy days, 15; boisterous, 6.—Mean of the hygrometer, 69¾.

The barometrical column during the whole of this month has been incessantly in motion, as will be seen from the number of changes in its direction, which are nearly equal to the days in the period. The range was confined to four days, viz. from the 1st to the 4th; on which last-mentioned day it was depressed nearly a full inch; but on the 10th, having nearly regained its former elevation, a violent storm of wind and rain from the N. on the 11th caused another depression of nearly an inch and a quarter.

The temperature for the most part has been very high for the season, and the variations trifling; indeed, in the minimis there has been a very sensible approximation to the mean.

The amount of rain nearly agrees with that for the last month: the greater part having fallen by night.

A very brilliant and splendid display of the Aurora Borealis was observed here in the evening of the 8th, and was followed by much wind and heavy rain. On the 25th and 26th, between eight and nine, p. m. the moon exhibited a very bright corona, encircled by a distinct double halo, which continued each time above two hours. These appearances were succeeded by a most tremendous hurricane from the W. during the whole of the 27th, accompanied between five and six, a. m. with loud thunder and vivid lightning. A very large solar halo appeared on the 21st, and was almost immediately followed by a violent gale from the S.W., and the heaviest shower of snow, mixed with hail, we have observed during the present winter.

New Malton, Feb. 2, 1817. J. S.

III. *On the Rat d'eau.*

(To Dr. Thomson.)

SIR,

I hope you will not regard the following observations on a singular phenomenon obtrusive, and unworthy a place in your Journal.

The phenomenon to which I allude has, I believe, been described by travellers before; but as it occurred to me during a recent tour through the South of France, I cannot resist the temptation of offering you a few desultory remarks upon it.

I refer to that singular occurrence in the river Dordogne which is commonly called the *mascaret*, and which is known in that part of France by the name of the *rat d'eau.* Major Rennel, in his account of India, has mentioned a similar phenomenon having been observed in the Ganges; and Constantine has described it as occurring in the river of the Amazons.

After a long continuance of dry weather, by which the waters of the Dordogne are very much reduced in quantity, we perceive at that part of its course where it mingles its waters with those of the Garronne this appearance presenting itself, as a huge mass of water somewhat resembling the form of a tun-barrel, which rolls from one side of the river to the other, at one time disappearing, and at another rising again with increased dimensions and violence, and proceeding up the river to the distance of about 22 miles.

As soon as its approach is indicated, both men and cattle retire from its banks.

From the suddenness of its appearance, and the violence with which it moves, it is often productive of serious evils. It has been frequently known to tear up by their roots trees which were growing on that side of the river to which it may have rolled, to sink or destroy boats, and to break down the banks of the river.

The seafaring men who reside at the mouth of the river can generally predict the occurrence of the *rat d'eau*, from observing the depression in the river, and the force of the flowing tide. From these circumstances they are generally able to escape those unpleasant and dangerous consequences to which this event gives rise.

This remarkable phenomenon usually presents itself first opposite to the village of Bec d'Ambes. From this place it proceeds up the river, suffering a variety of changes in its appearance, till it reaches the town of Libourne, where it roars with apparently increased impetuosity, agitates the waters of the river to a considerable extent, and at the same time suffers a very considerable diminution in its size and in its force.

This singular occurrence in the river Dordogne may doubtless be attributed to the combined operation of several causes, of which, however, the sea appears to be the most essential.

At the flow of the tide its waters are conveyed by the Gironde to the mouths of the rivers the Garronne and the Dordogne. Here the bed of the Garronne being considerably diverted out of the direction of the flowing tide, and the Dordogne being very favourably situated with regard to the Gironde, it (the Dordogne) receives a greater abundance of waters, which, entering with great rapidity, and penetrating very far in the form of immense waves, are thrown from side to side, and assume a variety of singular appearances. The diversified forms which the mascaret exhibits may be ascribed to the rapidity of the current of the river, to its numerous turnings, to the resistance which it meets from the sand-banks, and to a variety of other concurrent causes.

Yours respectfully,

T. W.

Edinburgh, Nov. 15, 1816.

IV. *Comparison of the Temperature of the Air in both Hemispheres.*

Latitude.	Corresponding months.	Mean temp. of the month.	
		Southern hemisphere.	Northern hemisphere.
0°—15°	December	82·4°	
	June		83·3°
18	October		78·7
	April..........	81·5	
2—26	January		66·74
	July	72·5	
	September		68·9
	March	69·44	
34	December		59·72
	June..........	56·84	
	February		62·6
	August	62·24	
43	July		64·76
	January	59·36	
48	June..........		63·68
	December	44·6	
53	July		56·3
	January	43·16	

Humboldt's Personal Narrative, ii. 85.

V. *Height of the Barometer.*

I have for some time past been anxious to draw the attention of meteorologists to some improvements in the mode of registering the height of the barometer, which would add greatly to the value of their tables.

1. It is well known that mercury expands by heat, and contracts on the application of cold. Hence the height of it in the tube is affected, not only by the pressure of the air, but by the temperature. Let us suppose, as stated by the Committee of the Royal Society, that the apparent expansion of mercury in glass is $\frac{1}{11500}$ of every degree of Fahrenheit's thermometer; and let us suppose two simultaneous observations made in two places, in one of which the thermometer stands at 32°, and in the other at 72°; the height of the barometer in the latter place would exceed that in the former by one-tenth of an inch, owing entirely to the difference of temperature. Hence, to enable us to compare the height of the barometer in one place with its height in another, it would be necessary to reduce the column of mercury to the length which it would occupy if the temperature were 32°. This reduction should always be made when the barometrical height is marked down in

the table. It could easily be done by having a small table of corrections ready drawn up, and subtracting the requisite correction from the observed height of the barometer. The annual mean of the height of the barometer thus kept would give the height of the place without any correction whatever.

2. Unless the tube be wide, the mercury stands higher in it than it ought to do. Hence, to obtain the correct height of the mercury, we ought either to use a wide tube, or we ought to compare our barometer with one having a wide tube, to determine how much higher it stands than it ought to do, and apply the requisite correction when we write down our observations.

3. In the Ann. de Chim. et Phys., an excellent monthly journal, edited by MM. Arago and Gay-Lussac, there is published monthly the meteorological observations as kept at the Paris observatory. In this table the height of the barometer (reduced to the temperature of 32°) is marked at nine in the morning, at noon, at three in the afternoon, and at nine o'clock in the evening. From the monthly mean of these heights, it appears that the barometer is highest at nine in the morning, next highest at nine in the evening, lower at noon, and lowest of all at three in the afternoon. The proper hours, therefore, for marking the height of the barometer are nine in the morning and three in the afternoon; and the mean between these observations would give the true annual height of the barometer in any particular place.

Were these observations attended to, barometrical observations would be much more useful than they are at present; and a proper collection of them would give us a correct idea of the height of the different places where they are kept above the level of the sea. It seems clear that barometers begin to rise and fall simultaneously over a very great portion of the globe at once.

VI. *Singular Experiment.*

(To Dr. Thomson.)

SIR,

The following curious and mysterious experiment, which I have several times performed, you may perhaps consider of sufficient interest to occupy a place in your *Annals*:—

Let a sixpence be fastened by means of a loop to a piece of thread, and the other end held between the first finger and thumb. Place the elbow upon a table within a few inches of a clean glass tumbler, in such a manner that the piece of metal may be suspended rather higher than the centre of the glass. If it be held quiet, it will soon begin to vibrate, which will increase to that degree as to cause it to strike the sides of the glass. So far it is singular. But the mystery consists in the number of times it strikes the glass always corresponding with the hour last struck by a clock. If the experiment should be tried a little before one o'clock, it will strike 12 times, and then stop; if a few minutes after it will only strike once, and almost instantly cease to vibrate.

This appears to me extremely curious; as the cause (which I should presume was electrical), one should think, would possess the same power ten minutes before as ten minutes after one o'clock; and what is still more curious is, that, having produced the effect related, it should almost instantly cease to act, as the piece of metal soon becomes stationary, and must be removed from the glass before it will again vibrate.

Very little attention appears necessary to perform this experiment; although it will not always succeed. A sixpence, or piece of metal rather heavier, will in general be found to answer.

It will give me great pleasure to see your opinion and theory, or that of any of your numerous readers, respecting this curious phenomenon, in a future number of your *Annals*.

I am, Sir, your obedient servant,

Barnet, April 2, 1817. Thos. S. Booth.

The phenomenon observed by Mr. Booth will naturally bring to the recollection of electricians the experiments of Mr. Stephen Gray of the revolution of small balls held in the hand by a string round large ones, and always in a direction corresponding with the motion of the sun. He bequeathed these experiments at his death as a kind of legacy to his electrical friends. The subject was soon after investigated with all the requisite care by Mr. Wheeler, who succeeded in demonstrating that the revolutions in question were not owing to any electrical property whatever, but to the voluntary action of the hand that held the string. Mr. Gray, though an acute man, was not aware that he exerted any such voluntary power; and it was by no means an easy task for Mr. Wheeler to ascertain this to be the true cause. I have no doubt that the motion of the sixpence in the experiment of Mr. Booth is owing to the voluntary action of his finger and thumb; and that he will be able to satisfy himself that this is the case by varying his trials, and by resolving beforehand that the sixpence shall strike some hour different from that which has struck last.—T.

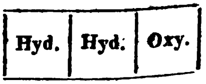
VII. *Further Improvement in Brooks's Blow-pipe.*

(To Dr. Thomson.)

DEAR SIR,

I, in a former number, suggested an idea for the improvement o Mr. Brooks's blow-pipe. If you think proper to give insertion to the following in your Journal, you will greatly oblige me.

Instead of having the apparatus made with two gasometers, I beg leave to suggest the propriety of having them formed in this manner: the hydrogen, being confined in two reservoirs, will in all probability tend in a great measure to prevent explosion. I trust you will make a trial of the propriety of this idea. The only objection I am aware of that can be brought



against it is, that the oxygen being possessed of a greater degree of electricity than the hydrogen, it may issue from the compartment in which it is collected, with greater velocity.

R. H. K.

VIII. *Experiment with a bent Gunbarrel.*

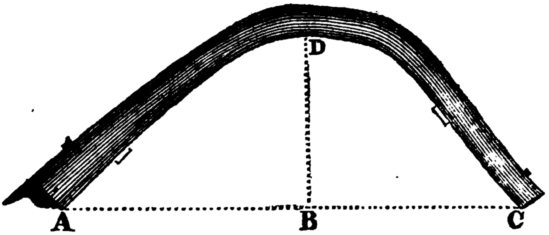
(To Dr. Thomson.)

DEAR SIR, *Swineford, March* 12, 1817.

When our regiment marched from Belfast last spring, a soldier with his musket slung over his shoulder was adjusting some baggage on a loaded cart. While doing this, the cart moved on; and as he endeavoured to get clear from it, the muzzle of his musket got entangled between the spokes of one of the wheels. The man would have been killed if the sling had not given way and liberated him. The stock was broken to pieces, and the barrel was bent into a curve not unlike a semicircle, in which state I found it among some lumber in our armourer's shop at Castlebar in the month of Nov. last. On learning the history of this crooked barrel, I examined it very carefully; and finding no flaw in the bend, it occurred to me to try whether it might be fired in this state. Some of our gentlemen having facetiously predicted that the ball would certainly go at least three times round the yard, I took many precautions in making the first experiment, which, however, were not at all necessary. The breech plug was unscrewed, a ball cartridge put in at the breech, and the plug again screwed tight. The barrel was then placed on the ground, and a great weight of stones and other matters laid upon it. A train was laid from the touch-hole to the door of the forge, from which I set fire to it with a touch of a red-hot iron. It went off with a great report; and the ball went through an old barrow, apparently with as much force as if the barrel had been straight. The recoil, which was expected to be violent, appeared to have been trifling, as none of the weights which lay on the barrel were displaced; and the barrel, which we expected to burst, did not appear to be at all injured by the explosion. The experiment was repeated three times, with the same results, only in the last the barrel (being laid on the ground without any weight upon it) made a jump of about five yards from its station. I have, however, ascertained that a straight barrel will do the same.

In the beginning of last month it occurred to me to make another experiment with this barrel. I made the armourer drill a touch-hole in the hollow of the bend, as nearly in the middle as possible. The breech plug was lost by this time, which indeed suggested the idea of the experiment. I took a common ball cartridge, and put a second ball into it, so as to have a ball at each end. This double-shotted charge was put into the barrel, and, by means of a strong bent wire, pushed so far as to place the middle of the powder opposite to the touch-hole in the bend. It was then fired by means of a train. Both balls appeared to have had great force. The ball from the muzzle went clear through a sort of flag slate, about three quarters of an inch thick, which was placed before it. The ball from the breech struck the ground, which it ploughed up, and then hit a large stone, which altered its shape. The first ball, after passing through the flag slate, was found at the bottom of a wall, which it had also struck, and which had flattened it nearly into the form of a penny piece.

Probably the great tenacity of the iron of which gun-barrels are made is the only thing of practical use to be deduced from these experiments. I am persuaded, however, that many of your readers will think them curious: and if you should think fit to give them a place in the *Annals*, the accompanying sketch will give a clearer idea of the form of the barrel than any description. It is to be observed, however, that the curve is not all in the same plane; for when laid on a surface, it only touches from A to D; the remaining portion rises gradually in a kind of spiral curve, until the muzzle, C, is about five inches clear of the surface; so that the ball had its direction *twice* altered before getting out.



	Ft.	In.
Length of the barrel straight	3	4
Length of the line A B C	2	7
Length of the line B D	0	10½

I remain, dear Sir, yours most truly,
J. Menzies.

IX. *On the Introduction of Vaccine Matter into America.*

(To Dr. Thomson.)

SIR,

In the last number of your *Annals* a correspondent under the signature of G. expresses his surprise to find it stated in my Memoirs of the Life and Writings of the late Dr. Lettsom, that the vaccine lymph was first sent across the Atlantic by Dr. L., and consigned to the care of his friend Dr. Waterhouse, of Cambridge, Massachusetts, from whence it *spread* through the United States. This is said to be untrue; that "vaccine lymph had been previously sent by Dr. George Pearson to Dr. Chichester, now a resident physician at Bath, but at that time in very extensive practice at Charles-town, in South Carolina." Reference is also given to the Phil. Mag. vol. xvi. p. 252, for the particulars of the vaccination of *one* individual in the winter of 1799 by Dr. C.

Nothing can be further from my wish than to attribute that to any individual which does not appear to be justly his due; and had I not been informed by the *highest* authority on this subject, that Dr. Lettsom had been the first to transmit to America this inestimable treasure, I certainly should not have ventured to state it in my publication. The notice of your correspondent has, however, induced me to examine a number of letters written by Dr. Waterhouse to Dr. Lettsom, and the notes of letters transmitted by Dr. Lettsom to Dr. Waterhouse, to be convinced of the truth or error of the statement I have made. From this examination I am free to confess that Dr. L. does not appear to have *first* sent the vaccine lymph across the Atlantic. I subjoin extracts from these letters, that your readers may draw their own conclusion with respect to dates. I cannot, however, forbear expressing some degree of surprise that the practice of vaccination was not followed up by Dr. Chichester; for it is rather singular that no mention of his name occurs but in the statement contained in the Phil. Mag., unless it be in the writings of Dr. George Pearson, which I happen not to be in possession of, and to which it is at present not in my power to refer. Dr. Waterhouse, on the contrary, is frequently alluded to by various writers. He was the active promoter of the practice in America; the person to whom the members of the government applied for lymph, and for directions for its use, and to whom also his professional brethren looked for information on this subject. He seems also, from the following extracts, justly to feel the responsibility of his situation on the occasion. If, therefore, Dr. W. was not the *first* (which, I confess, appears to me doubtful) who vaccinated in America, it is but due to admit that it was by him that the practice *spread* through, and was finally established in, the United States.

(Dr. W. to Dr. L.)

Cambridge, April 10, 1799.

"I received with great satisfaction your letter of the 24th Nov. with Dr. Jenner's and Dr. Pearson's publications on a new, curious, and extremely important disease. I directly threw an account of it into the newspapers, a copy of which I here enclose. I should be highly gratified by more information respecting this epizoatic disorder, and of further trials on the human kind. As such a distemper has never been heard of in this country, it excites the public curiosity as much as any thing that has occurred in the medical line since my remembrance."

Nov. 14, 1799.

"I here enclose a letter for Dr. Woodwille, as it is making the same request I did to you respecting some cow-pox matter.* I send it opened, and wish you would be so kind as to put a wafer in it, and suffer the penny post to convey it to him. The curiosity, nay anxiety of the public, especially of parents, on this subject, is very considerable. We indeed feel anxious ourselves, as four of our six children have never been innoculated."

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* From this passage it seems probable that Dr. W. had previously solicited vaccine lymph from Dr. L. No letter, however, containing a request of this kind is preserved.

Nov. 13, 1800.

"I am in no small tribulation for want of the vaccine matter. I introduced it into this country; but some how or other it has depreciated in my hands. It fails in more than half I have innoculated for several weeks past. I never received any but from Dr. Haygarth, which was last June.

"I have never been able to procure Woodville's last publication on the cow-pox. Are there any good practical treatises recently published on this subject? As I was the first who introduced it here, I am applied to from all quarters, but am chagrined almost to sickness, because I have no confidence in the matter I possess. The vaccine poison has become milder by passing through a number of the human species, or else the cold weather has deprived it of half its venom. As soon as I receive fresh matter from England, I will directly innoculate a cow, by way of obtaining active matter from the fountain head.

"I have had several instances in the cow-pox where the symptoms came on pretty violently in 24 hours. In many instances I am puzzled to know whether the patient has really gone through the disease, so as to secure him from further infection. My situation is peculiarly perplexing; for should any unfortunate case occur under any practitioner, I shall bear the blame of it. I have diffused the matter all over the country, and am conscious that it has degenerated and become spurious. Applications by letter and otherwise crowd upon me every hour, and almost every minute, to solve doubts, give directions, and console disappointments; and I have no person to apply to myself, for the information which I feel I myself stand in need of. I have Fenner's work, first and second part; Woodville's first publication; and Pearson's first pamphlet; and the second volume of the Medical and Physical Journal; and could wish that Mr. Mawman would send me any thing and every thing that has or may come out in the course of the winter *which you can recommend.*"

Dec. 13, 1800.

"As I know not Dr. Jenner's address, I have enclosed a letter which I would thank you to forward to him as soon as possible. I have written to him on a subject in which I am deeply interested, I mean the cow-pox. You already know, perhaps, that I introduced that distemper here, and led the way in its innoculation, and that very much to my advantage; but it has lately worked very perversely, and occasioned me much perplexity. Since the cold, raw weather of November came in, the *matter* has *deteriorated* in my hands, and in the hands of every one else, so that almost all the cases that have lately occurred have proved *spurious*; and unless I can obtain a fresh supply of the vaccine matter early in the spring, the innoculation for it will sink into disrepute, and I myself come in for a large portion of the disgrace.

"Judge of my anxiety when I say that I am conscious that more than one hundred practitioners in different parts of New England are at this time inoculating with *spurious* matter, while the small-pox is pervading one of our sea ports, and every unfortunate case will be traced up to me, the originator of the practice in America. I am the only person who ever succeeded in obtaining efficient matter from England, which I had from Dr. Haygarth, and the activity of this matter *seems worn out by passing through a number of the human species;* and unless I obtain a fresh supply from England, the business which promised so fair here will stagnate. I have, therefore, written to Dr. Jenner for his advice and assistance. So I have to Dr. Pearson; and hope by the return of the Galen or by the Minerva to get a supply of the matter I stand so much in need of. I have just received an order from the War Department to supply the military surgeons with the vaccine matter, and directions for inoculating the different corps of artillerists and engineers stationed in the various parts of New England."

(Dr. L. to Dr. W.)

London, March 28, 1800.

"Sent him fresh cow-pox matter."

Dec. 24.

"Sent him vaccine matter in a glass bottle, from the Vaccine Institution, as well as two planes of glasses, one from Dr. Woodville, and the other from Mr. Johnson. Explained Dr. Woodville's, Dr. Pearson's, and Dr. Jenner's opinions, respecting the wearing out of the matter."

Feb. 19, 1801.

"Sent him vaccine matter in a glass vessel from myself, with a letter and vaccine matter from Dr. Pearson."

I have the honour to be, Sir,
Yours very respectfully,
Bolt-court, Fleet-street, April 16, 1817. T. J. Pettigrew.

X. *On the Introduction of the Antiphlogistic System into Great Britain.*

(To Dr. Thomson.)

SIR,

"My atomic theory of chemistry is so mathematically correct, that all visionary hypotheses fell prostrate before it, and *it was from it* ALONE *that the phlogistic doctrine received its fatal blow.*" (Higgins, Phil. Mag. p. 364, Nov. 1816.)

In the paper from which the above extract is quoted you are rated with vivid animosity on a charge of partiality to Mr. Dalton, and injustice to the author, respecting the honours of the atomic theory.

Whether this author has obtained less honour from you than his due, respecting the *atomic theory*, I shall not inquire; but that, in the above quotation, he arrogates much more than his due, respecting the theory of *phlogiston*, I mean to make appear in this paper.

This famous view of the atomic theory, it seems, has been long out of print, so that we cannot hear it speak for itself; but its author informs us that it was an excellent treatise, and that the date of its publication was the year 1789.

I have no need to question the accuracy of these statements in order to deprive this author of the *sole* honour of exploding the phlogistic doctrine. Before he preferred a claim to this honour, he ought to have known all the advances of chemical science in Europe and America. Seven or eight years before this author's date of his theory, in 1781, Lavoisier had detailed, in the Memoirs of the Academy of Sciences at Paris, his discovery of the *composition of water* (Lavoisier's Chemistry, chap. viii. Introduction); and early in 1789 his entire system, with the new nomenclature, was not only published in French, but also in an English translation. No chemist who adopted this system would either need or find room for the doctrine of phlogiston. Lavoisier, therefore, not only lays claim, but lays a previous claim, to the honour of exploding the phlogistic doctrine.

But, in justice to my Alma Mater and *quondam* teacher, I am induced to bring forward another prior claimant to this honour. As appears in his paper, this author dates his claim in 1789. I can inform him, however, that during the winter of 1786—1787, I attended the chemical lectures of Dr. Irvine in the University of Glasgow. At that time, *i. e.* two years earlier than his own date of his theory, I can assure our author that, so far as the doctrine of the teacher, and the chemical creed of many of his pupils, could effect any thing, phlogiston was dismissed to the family vault of all the Capulets, to rest in peace with *Elixir vitæ*, *Spiritus rector*, *Archæus*, and their fellows. The opposite theory of combustion was fully and zealously stated and illustrated; and also exemplified by the combustion of alcohol, phosphorus, and sulphur; and in the account of the manufacture of sulphuric acid, as well as in the combustion or calcination of metals. At the conclusion of the same course of lectures, Dr. Irvine detailed also Lavoisier's leading facts and views respecting the composition of water. Besides, this teacher did not state his views, in opposition to phlogiston, as new or recent, but as having been entertained for a considerable time. He also stated that, at this date (1786—7), the Professor of Chemistry in Edinburgh had adopted the same views, and relinquished the phlogistic theory.

The other professors in Glasgow University soon followed Dr. Irvine in adopting the new doctrines on combustion. This eminent chemist died (of fever, I think) during 1787; but the University, on May 1, 1788, I think, announced as a prize essay, "The best account of the modern discoveries respecting the composition of water;" prize—a silver medal; the candidates to be such students as had finished the logic, ethic, and natural philosophy classes. As few such students had turned their attention to chemistry, the same prize essay was again announced May 1, 1789, and adjudged May 1, 1790. Of this essay I possess the first copy. It was composed during the session 1789, 1790; and contains Lavoisier's doctrines respecting caloric, carbon, oxygen, hydrogen, &c.; with the application of these doctrines to explode the theory of phlogiston, and to explain a variety of phenomena.

Although this author's atomic theory had been as extensively known as Lavoisier's work, the above facts would subvert his claim to the *sole* honour of exploding the phlogistic doctrine; but as this work has been long out of print, and has never been till now heard of, by many readers of chemical works, the claim seems still more extravagant.

I am far from wishing to depreciate the character of this author. I believe he is a distinguished chemist: but by claiming more than his due, he will be in danger of reducing his reputation, as a man, below its proper level.

I am, Sir, your most obedient servant,

Glasgow, March 12, 1817. JAMES WATT, M. D.

XI. *Sale of Minerals.*

(To Dr. Thomson.)

SIR, 25, *King-street, St. James's, March* 19, 1817.

Finding that you have occasionally allowed a place in your pages to notices respecting sales of minerals, I beg leave to inform you that I am preparing one which is to begin about the end of April. I cannot speak of its duration; but I purpose to divide the whole into sections of three days each, to take place at short intervals. Very often having been asked after what manner I would arrange my two *private* collections (which I have some intention to place according to Werner, modifying them, however, as I may see proper) I have been led by those questions to lot my sale agreeably with Werner's system, deviating from it only in those cases where chemistry does not permit the families to be followed up, conformably with the opinions of other enlightened naturalists. That the patience of the collectors may not be wearied, I shall intersect each day's sale with labelled lots, not referring to the system, but always consisting of some interesting and scarce substances. On former occasions the earlier days' sales principally consisted of products, which I sacrificed with pleasure in favour of young collectors and of trade; but I confess that I found it somewhat amusing to perceive combinations amongst some of the purchasers, who on such an occasion, about two years ago, were assured in the sale-room that it would be my last attempt, and much commiseration was handed about as to my exit. The fears of those compassionate members of society were, however, quieted, upon bringing forward the labours of my year's collecting, which enriched the cabinets of those ladies and gentlemen who know how to estimate the science, and have just feelings! Allow me, therefore, amidst such different opinions and principles, to explain the course which I find it necessary to pursue, viz. that I have no objection against making many a sacrifice; but at the same time I will not submit to leave unprotected the *whole* of the property which I offer. To prevent so ruinous a case, the auctioneer (Mr. King) will at once put up with my ultimatum *those* lots which do not allow me to become the victim of the times, of combinations, or of illiberal motives! I have heard that much is expected from me, when making a sale, in point of quality and choice. I too may reasonably expect some sympathy; and I do rely upon the protection of the public. Some curious facts may at a future period be developed in proof that ideas exist in this country regarding mineralogy which are unknown abroad; and here I merely suggest to any of your readers the question how it comes and has happened that since Mr. Forster, my late uncle, left this country for Russia, in 1795; but more particularly since the death of Mr. Greville, in 1809, and since the greater part of the nobility have ceased collecting, the majority of actual collectors have come to the singular conclusion that the commerce of minerals is contemptible? In other cities of Europe the toils of my late uncle, as well as my own, have inspired a widely different feeling.

Whilst upon this subject, I need only to say, that a *true* dealer in minerals is, *ipso facto*, a merchant, embarking his capital in all parts of the known world, and honourably upholding the pursuits of science, which must ever be inseparably connected with legitimate commerce; and that there are few exceptions where collectors are not themselves, publicly or privately, dealers. For the truth of these remarks I appeal to your readers. Consequently, if I remain in England, I trust that, for the future, the collectors and the friends of mineralogical science will not suffer illiberal prejudices to damp the ardour of a pursuit which, calling for continual sacrifices, by *absorbing* instead of making a fortune, demanding unwearied application, and attended with numberless difficulties and risks, surely constitutes a powerful claim upon impartial judgment and manly feeling! I hope there is not any presumption in the statement of my opinion that the removal of my large private collection would prove a real loss to this country. It may be permitted me to say so, when it is acknowledged throughout Europe that *there* are to be found the fruits of 60 years mineralogical labours, comprising the period when my late uncle, Mr. Forster, commenced his career, up to the present hour, wherein I have unremittingly followed his steps, determined to procure the finest of substances, and regarding expense as a secondary consideration. In all humility, and with the will of Providence, I can reckon that in 18 months' time my collection will have risen to such a grandeur as may satisfy my ardent desire, that in the bosom of this country I may repose the most select and noble mineralogical products known in the world.

An event has taken place in France which may shortly deprive its capital of its finest private collection (that of the Marquis de Drée's), and of which the Parisians had just reason to be proud. To replace it, upon my own terms, with mine, which is well known on the Continent to be infinitely superior, and to unite my fate with that my dearest pursuit, under the highest sanction in France, would cost me but the writing of a letter. Much more than this I might expect from the munificence of the Emperor Alexander, whose pride it is to advance the sciences in his empire, and to whom my late uncle was sufficiently known, were I to yield to a similar impulse, which is often kindled by the remembrance of the years I passed in Russia. Why, then, should I not be entitled to the impartiality of the mineralogists in this country? More I do not claim; and were it only for the policy to secure some day to Great Britain so fine a national property, the collectors and scientific men should rather rejoice to forward views which are truly those of the love of the science, and not of interest.

I have the honour to be, Sir,
Your most humble and obedient servant,
HENRY HEULAND.

P.S. I just learn from M. Brochant de Villiers (whom you did not mention as one of the new members of the French Academy for the mineralogical department) that all possible influence is used to preserve to France the collection of the Marquis de Drée; and the Savans have petitioned the King accordingly.

XII. *Correction of a Mistake in Mr. Donovan's Essay on Galvanism.*

(To Dr. Thomson.)

SIR,

I beg the favour of correcting, through the medium of the *Annals of Philosophy*, an error in my Essay on Galvanism recently published, which entirely destroys the sense and force of the experiment, and which heretofore escaped my attention. The passage beginning at p. 277, line 26, runs thus: "the other wire was inserted into a disc of copper, in a similar manner." It should be read, "the other wire was inserted into a disc of cork, in a similar manner."

I hope that such readers of your *Annals* as have copies will take the trouble to correct the error.

I am, Sir, with respect, &c.

Dublin, March 13, 1817. M. DONOVAN.

XIII. *An Anthelion observed at Tottenham.* By L. Howard, Esq.

On the 19th ult. I had an opportunity of observing that very rare phenomenon the *Anthelion*. It was formed on the perpendicular part of a lofty dense *Cumulostratus,* which happened to present, in the N.E. at near five p. m. a surface directly opposed to the sun, which perceptibly reflected *an image of the disk* at the same apparent height from the horizon. In a few minutes, and almost as soon as I had satisfied myself of the fact, it was obliterated by a new protuberance in the cloud destroying the direct reflection. An Anthelion observed by Swinton near Oxford in 1762 is described, with a figure, in Vol. XI. of the Phil. Trans. Abridged, p. 532, to which the reader is referred; but in the present instance, the whole cloud being bright, the contrast between the general surface and the sun's image was probably less striking than in Swinton's observation.

Tottenham, Fourth Month, 22, 1817.

XIV. *New Earth.*

Professor Berzelius has just discovered a new earth, to which he has given the name of *thorite,* from the Scandinavian god *Thor.* I shall take a future opportunity of laying an account of its properties before my readers.

XV. *New Method of Freezing Water.* By Professor Leslie.

(To Dr. Thomson.)

SIR,

My early experiments had proved that garden mould and decayed green-stone, which constitute the basis of our richest soils, acquire, when thoroughly dried, a power of absorbing moisture almost equal in intensity, though not in extent of action, to the energy exerted by the concentrated sulphuric acid itself. Circumstances recently drew my attention again to this curious subject. I directed my servant to gather some of the shivery fragments of porphyritic trap from the sides of the magnificent walk now forming round the Calton Hill, and having pounded it grossly, to roast it moderately before the kitchen fire under a tin oven, and then throw it into a wine decanter with a glass stopper. In this state of preparation, the powder was afterwards carried to the College; and, at a lecture some days since in the natural philosophy class (which, in addition to my own mathematical classes, I have been teaching this session during the absence of Professor Playfair in Italy), I took occasion to show the influence of its absorbing power on my hygrometer placed over it within a small receiver of an air-pump. The liquor of the instrument fell from 90° to 150°, and rose again to 130, where it stood for a minute, while the lint covering the wetted ball was turning whiter, and evidently freezing, but again regularly descended to about 320°, the temperature of the room being only about 55° of Fahrenheit. A cold had thus been produced corresponding to 5° below the zero of that scale; and I did not hesitate to propose on the instant to employ this new agent in freezing a small body of water. I transferred the powder into a saucer about seven inches wide, and placed a shallow cup of porous earthenware three inches in diameter at the height of half an inch above it, and covered the whole with a low receiver. On exhausting the air from this receiver till the gauge stood at two-tenths of an inch, the water in a very few minutes was converted into a cake of ice. With the same powder, above an hour afterwards, I repeated the experiment in the presence of one or two friends; and in the space of three minutes after the receiver was placed, a larger body of water began to congeal, and was quickly consolidated.

It appears that such dried earth will absorb the 50th part of its weight of moisture before its absorbing power is diminished one half, and the 25th part of its weight before this power is reduced to one fourth. When completely saturated with humidity, it may hold near a fifth part of its whole weight. Since, therefore, the quantity of heat abstracted by the process of evaporation is adequate to the congelation of about eight times an equal weight of water, the dry pulverized green-stone or garden mould is capable of freezing more than the sixth part of its weight of water. To ensure success and expedition, however, I should prefer rather a larger proportion of the powder. The contents of two quart decanters, for instance, poured into a saucer of a foot diameter, might be employed to freeze half or three quarters of a pound of water in a hemispherical cup of porous earthenware. This powder is capable of acting still, though with feebler effect. It should, therefore, after each process, be dried again, which will restore it completely to its former energy. Such partial drying will be quickly and easily performed; and in hot countries the mere exposure of the powder for a while to the sun may be sufficient. Ice may, therefore, be procured in the tropical climates, and even at sea, with very little trouble, and no sort of risk or inconvenience.

I mean to pursue this curious subject, and to institute immediately a series of experiments on different compound earths; and should the results prove interesting, I will not fail to transmit them to you.

I am, dear Sir, very sincerely yours,

Edinburgh, April 17, 1817. JOHN LESLIE.

ARTICLE XIV.

New Patents.

JOHN WELCH, of Preston, cotton-mill roller-maker; for an improvement in making rollers used in spinning wool, cotton, silk, flax, tow, or any other fibrous substances. Aug. 3, 1816.

SAMUEL NOCK, of Fleet-street, London, gunmaker; for an improvement in the pans of locks of guns and fire-arms. Aug. 12, 1816.

ROBERT TRIPP, woollen-draper, Bristol; for an hussar garter with elastic springs and fastenings, and also elastic springs for pantaloons and other articles. Aug. 14, 1816.

James Neville, of Wellington-street, Northampton-square, London, gentleman; for new and improved methods of generating and creating or applying power, by means of steam or other fluids, elastic or non-elastic, for driving or working all kinds of machinery (including the steam-engines now in use), and which are applicable also to the condensing of steam, and other aqueous vapours, in distillation or evaporation, and are useful in various manufactories and operations where heat is employed as an agent, or where the saving of fuel is desirable. Aug. 14, 1816.

Edward Biggs, of Birmingham, brass-founder; for improvements in or on the machinery used in the making or manufacturing of pans and *stails* of various kinds. Aug. 14, 1816.

William Moult, of Bedford-square, London; for improvements on his former patent for an improved method of acting upon machinery, bearing date May 23, 1814. Aug. 14, 1816.

Jean Samuel Pauly, of Brompton, engineer; for a machine for making of nails, screws, and the working all metallic substances. Aug. 15, 1816.

Robert Salmon, of Wooburn, surveyor; for improved instruments for complaints in the urethra and bladder. Aug. 19, 1816.

ARTICLE XV.

Scientific Books in hand, or in the Press.

Mr. Merrick has nearly ready for the press a Translation of Thenard's Treatise (1816) on the general Principles of Chemical Analysis, in one volume, 8vo.

Mr. Parkinson, of Hoxton, intends publishing, in the course of the month, an Essay on the Shaking Palsy.

Mr. William Phillips, author of the Outlines of Mineralogy and Geology, &c. will publish this month, in 12mo., Eight Familiar Lectures on Astronomy, delivered at Tottenham last winter to a numerous audience of young persons. It will contain the requisite diagrams and illustrations: and being intended for the initiation of the young, and for those who are unacquainted with the science, its numerous terms are as much as possible avoided, and such as cannot be avoided are fully explained in these lectures.

Dr. Wilson Philip is about to publish an Experimental Inquiry into the Laws of the Vital Functions, with some Observations on the Nature and Treatment of Internal Diseases.

Mr. Parkes has just published Thoughts on the Salt Laws, in which he presses on the attention of the Legislature the advantages which would be derived from a total repeal of the duties on that article.

Mr. Thomas Purton, of Alcester, is about to publish a Midland Flora, which will comprise descriptions of plants indigenous to the central counties of England: it will be illustrated by plates engraved by Mr. James Sowerby. [N.B. *In the* Annals *for last month the work itself was erroneously stated to be by Mr. J. S.*]

ARTICLE XVI.

METEOROLOGICAL TABLE.

1817.	Wind.	Barometer.			Thermometer.			Hygr. at 9 a. m.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
3d Mo.										
March 10	N W	30·10	29·91	30·005	47	28	37·5	75		☾
11	S W	30·10	29·94	30·020	53	39	46·0	66	—	
12	W	29·91	29·84	29·875	55	47	51·0	70	1	
13		30·15	29·91	30·030	55	42	48·5	65		
14	N E	30·22	30·15	30·185	51	30	40·5	73		
15	S E	30·22	30·16	30·190	50	27	38·5	70		
16	E	30·23	30·16	30·195	49	27	38·0	82		
17	S E	30·24	30·20	30·220	48	25	36·5	85		●
18	Var.	30·20	29·85	30·025	52	33	42·5	50		
19	N W	29·75	29·74	29·745	47	27	37·0	52		
20	N	29·88	29·75	29·815	34	24	29·0	47		
21	N	29·90	29·86	29·880	39	17	28·0	59		
22	S E	29·97	29·90	29·935	39	19	29·0	80		
23	S W	29·92	29·88	29·900	46	24	35·0	58	—	
24	W	29·88	29·72	29·800	55	39	47·0	80	—	
25	Var.	29·85	29·72	29·785	58	34	46·0	74	7	
26	Var.	30·00	29·85	29·925	52	34	43·0	52	·13	☽
27	Var.	30·05	29·92	29·985	44	27	35·5	62		
28	S W	29·92	29·76	29·840	50	38	44·0	72	4	
29	W	29·99	29·76	29·875	55	45	50·0	60	—	
30	N W	30·23	29·99	30·110	59	39	49·0	50		
31	N W	30·51	30·23	30·370	54	32	43·0	64		
4th Mo.										
April 1	S	30·51	30·37	30·440	56	36	46·0	67		○
2	S E	30·37	30·27	30·320	58	33	45·5	50		
3	E	30·33	30·27	30·300	60	37	48·5	65		
4	N E	30·33	30·30	30·315	56	34	45·0	70		
5	N	30·32	30·25	30·285	53	26	39·5	64		
6	N E	30·43	30·32	30·375	50	37	43·5	52		
7	E	30·37	30·20	30·285	52	30	41·0	60		
		30·51	29·72	30·070	60	17	41·5	64	0·25	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Third Month.—10. Fine, with *Cumulostratus.* 11. A mist, probably from the Thames, there having been much *Cirrostratus* at sun-rise in the SE: cloudy, p. m. with a few drops. 12. a. m. *Cirrostratus* in flocks: at evening a slight shower, with wind. 13. Fair: overcast with *Cumulostratus.* 14. This morning at eight the wind sprang up at NE, a gentle breeze, which, being propagated upwards, carried a veil of *Cirrostratus* off to SW: in the evening the sun's disk was curiously disfigured by the intervention of *Cirrostrati,* with vapour: after being divided, and afterwards crossed as by belts of this cloud, the lower portion came out much enlarged horizontally, while the part yet obscured became somewhat conical upwards. 15. a. m. *Cirrostratus:* misty to SW, after which light breezes and general cloudiness. 16. Hoar frost: fair: wind SE a. m., NE p. m. 17. A dripping mist, after hoar frost: then *Cumulus,* and the wind S. 18. Hoar frost, misty morning, SE: clear day: p. m. the wind SW, a smart breeze: clouds after dark. 19. a. m. Wind SW: *Cumulus,* beneath *Cirrocumulus* and *Cirrostratus:* p. m. windy at NW: *Cumulostrati* and *Nimbi,* with a little hail. 20. A gale at NNW, tending continually to go to N: a very scanty snow at intervals. 21. Very fine: *Cumuli* prevailed, which evaporated at sun-set: the roads quite dusty: wind tending to E, a smart breeze: night calm. 22. Hoar frost: hygr. noted at eight a. m.: very light breeze. 23. Hoar frost: fine day: evening obscured by *Cirrostratus,* which descended from above. 24. Some drizzling rain this morning. 25. Hoar frost: rain: a hail shower: p. m. the wind NE. 26. a. m. Overcast with *Cirrostratus:* small rain, p. m. 27. Very fine day: wind a. m. NNE, with *Cumulus* and *Cirrostratus.* 28. Wind S, a. m. with *Cirrostratus:* drizzling drain. 29. Temp. 50° at nine, a. m.: windy at SW. 30. Very fine morning, with dew: *Cumulus* beneath *Cirrocumulus* and *Cirrostratus:* a few drops of rain: a small yellow lunar halo: much wind in the night. 31. Windy: *Cumulus* beneath large *Cirri:* a lunar halo, white and of large diameter.

Fourth Month.—1, 2. Light driving mists, followed by fine days. 3. Hoar frost: rose-coloured *Cirri* at sun-set. 5. Cloudy: a few drops: misty night. 6. Hoar frost: *Cumulus,* with *Cirrocumulus:* windy. 7. Windy: SW by night, with mist.

RESULTS.

Winds for the most part light and variable, but on the whole Northerly.

Barometer:	Greatest height	30·51 inches.
	Least	29·72
	Mean of the period	30·07
Thermometer:	Greatest height	60°
	Least	17
	Mean of the period	41·5
Mean of the Hygrometer		64°
	Rain	0·25 inch.

The change from the turbid Atlantic air, which had for many months been flowing over us, to a dry transparent medium, was, from the commencement of this period, strikingly obvious to the sense. The sun assumed a splendour, and the moon a brilliancy, to which the eye had been long unaccustomed, and distant objects seemed as it were restored to the landscape. The mean of the barometer is the highest that has occurred to me since the spring of 1813: the *ten* dry days about the commencement of the period are the first that had happened in strict succession for twelve months; and there has not fallen so little rain in any lunar period that I have registered since the beginning of 1810. The evaporation has doubtless been excessive, and I regret that I have kept no account of it: the state of the hygrometer does not fully indicate this, on account of the misty mornings.

Tottenham, *Fourth Month,* 22, 1817. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

JUNE, 1817.

Article I.

Biographical Account of Hippolyte-Victor Collet-Descotils, Chief Engineer and Professor of Chemistry to the Royal Company of Mines; Member of the Institute of Egypt, of the Philomatique Society, of Arcueil, and of Encouragement; Correspondent of the Academy of Sciences and Arts at Munich; Associate of the Academy of Sciences and Belles Lettres of Caen, and Non-resident Member of the Agricultural Society of the same City. By M. Gay-Lussac.*

THE sciences have suffered a loss by the death of Collet-Descotils which cannot easily be repaired. A premature death has snatched him away in the midst of his career, after a degree of success which does him honour; but before he was able to accomplish every thing that was to be expected from his talents and his zeal for the sciences. Connected with him as we were by the closest intimacy, we consider it as our duty to offer a tribute to his memory. It will be impossible to console his friends and his family for his loss. While his labours, by recalling hopes that have been disappointed, will occasion regrets that will not be soon forgotten.

Collet-Descotils was born at Caen, Nov. 21, 1773. He began and finished his studies at the College du Bois, in the University of that city, under the direction of his paternal uncle. His father, a well-informed advocate, and First Secretary *de l'intendance* de Potiers, conducted him to Paris at the beginning of the revolution. There he had as his chemical teacher M. Vauquelin, then Professor at the Atheneum; and M. Charles was his instructor in physics. M. Vauquelin took notice of him, and inspired him with a passion for chemistry, which he cultivated himself even at that time with success. In a short time they were united together by the bonds of intimate friendship.

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* Translated from the Ann. de Chim. et Phys. iv. 213, February, 1817.

M. Descotils began his scientific career at the commencement of the troubles of the revolution; and towards the end of 1792 he was obliged to embark in quality of *novice* in a small vessel belonging to the state, from which he passed into a vessel stationed in the roads of Cherbourg. Soon after, by the advice of Dr. de Laville, for whom he always expressed a particular esteem, he resolved to stand candidate for the place of Eleve in the School of Mines, which Government had just re-established. He received information of his admission into that school at the same time with the news of his nomination to the situation of *aspirant* in the marine; but he did not hesitate in his choice. Renouncing the dangers of the sea, and preferring study, and the pleasures of a life of tranquillity, he returned to Paris, where he gave himself up entirely to his taste for chemistry.

In the year 1798 a great expedition was talked of, the object of which was unknown. MM. Berthollet and Monge, who were to form a part of it, proposed to Descotils to accompany them. The proposal was accepted; and Descotils, ignorant of his destination, but sure of acquiring information from these philosophers, so deservedly celebrated, gave himself up to his destiny; and, after a voyage of 40 days, found himself on the coast of Egypt.

During his abode in that burning climate, and in the midst of dangers constantly returning upon him, he gave himself up to several scientific researches, and he was one of those philosophers of whom the Institute of Egypt had to boast. On his return to Paris he got the management of the Laboratory of the School of Mines; and in 1809 he was raised to the rank of Engineer in Chief, which he so well deserved.

Notwithstanding the duties which Descotils had to discharge, his short career was marked by numerous experimental labours. We are indebted to him for many analyses of minerals,* in which he has shown a great deal of skill; but we shall merely mention such of his researches as have given him a distinguished place among chemists.

His most important memoir is relative to the cause of the colours which certain salts of platinum assume (Ann. de Chim. xlviii. 153). He has shown that these colours are owing to the presence of a particular metal, which has been since distinguished by the name of *iridium*, and which is found likewise in the residuum which platinum leaves when dissolved in acids. These results are of the greatest importance, because they have enriched chemistry with a new metal, remarkable for the variety of colours which its combinations assume, and they would be sufficient alone to transmit the name of Descotils to posterity.

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* Only a small number of these have been published. The rest were made for private individuals, or for the Administration of Mines. Among the latter are those of the French ores of tin, and particularly those of the department of the Haute Vienne, the results of which are very important, because they show that France possesses extensive tin veins, which may be wrought with advantage.

In the Memoirs of the Society of Arcueil (i. 370) Descotils has given a note on the purification of platinum, which has contributed to lower the price of this precious metal. He proposes to begin by alloying crude platina with zinc, and to digest the alloy in a state of powder in sulphuric acid to separate the zinc. The platinum then dissolves very readily in aqua regia. It presents the remarkable property of burning at a very gentle heat, and even of detonating like gunpowder when the proportion of zinc employed has not been considerable.

Mr. Chenevix had observed that platinum, precipitated from its solution by nitrate of mercury, and reduced in a crucible with a little borax, gives a well-fused button of metal about 17 times heavier than water. Descotils, on repeating this experiment, observed that platinum may be fused by means of borax without the assistance of mercury, and that, when it was dissolved in acids, boracic acid was obtained. This fusible platinum is a true boruret, very brittle, and having a crystalline texture. Other metals fused with borax presented the same phenomena as platinum. Hence we ought to consider Descotils as the first person that formed borurets.

While making these experiments on platinum, he observed that charcoal likewise has the property of combining with platinum in the proportion of two or three per cent., and of diminishing its density. (Ann. de Chim. lxvii. 86.)

We were ignorant of the cause of the infusibility of some varieties of sparry iron ore, and of the theory of the processes put in practice to render them fusible. Descotils showed by an exact analysis that sparry iron ore is not always the same, and that the refractory quality of some varieties of it is owing to the great proportion of magnesia which they contain. When these last are left for a long time exposed to the air, either before or after being roasted, sulphate of iron is formed, the acid of which combines with the magnesia, and the new formed salt is washed away by the rain-water to which the ore is exposed. From this theory he advised, in order to accelerate the separation of the magnesia, to water the heaps of roasted ore with water holding sulphate of iron in solution, a salt which may be easily procured by gently roasting, and then exposing to the air the pyrites which usually accompany sparry iron ore. The advantages of this mode are obvious, as it enables us to remove the magnesia in a much shorter time than usual. (Jour. de Min. xxi. 277.)

There exist certain kinds of iron ore, which different mineralogists have united under the name of *clay-iron-stone*. Descotils has proved that this ore is an earthy carbonate of iron, the situation of which is remarkable, as it almost always accompanies coal, and as in the places where it occurs alone the beds possess the characters of those which usually contain that combustible. (Jour. de Min. xxxii. 361.)

Descotils, whose views were chiefly directed towards metallurgy, had been struck by the enormous loss sustained when galena is decomposed by the common way. He examined (in the Memoirs d'Arcueil, ii. 424) the influence of the gases on the decomposition and volatilization of this mineral. He has given the theory of the operation; and has shown that advantage would result from decomposing galena by a substance which would absorb the sulphur without producing any gaseous body.

Water mortar had been the object of the researches of several chemists. Bergman ascribed the property which it had of drying under water to the presence of about two per cent. of oxide of manganese. Guyton had observed that different substances gave lime the property of drying under water: he had even pointed out a method of making water mortar artificially, by calcining together a mixture of four parts clay, six parts of black oxide of manganese, and 90 parts of good lime-stone, reduced to powder. (Ann. de Chim. xxxvii. 259.) But Descotils ascribes the property chiefly to the silica, which occurs in considerable proportion in the lime-stones that form water mortar. He made the important remark that the silica does not dissolve in acids before the calcination of the limestone, while after that calcination it dissolves almost entirely. These results, while they give an exact idea of water mortar, point out the way of forming it artificially, and explain the solidity which it acquires so speedily under water. (Jour. de Min. xxxiv. 308.)

Descotils had employed himself much in the examination of alum, and the different sulphates of alumina. He had obtained results which he informed us were very remarkable; but he has left nothing in writing upon the subject. It was while engaged with alum that he passed a current of chlorine through sulphate of alumina and ammonia, and discovered the chloride of azote, the existence and nature of which was first announced by M. Dulong. He had observed the property which this fulminating compound has of becoming solid by application of cold, but had deferred a particular investigation of it. (Jour. de Min. xxxiii. 351.)

As Chief Engineer of Mines, Descotils went on several important missions. He visited the famous alum mines of Tolfa, on which he made a set of observations, which will appear in the first volume of the Annales des Mines. We regret much that he was unable to execute a project which he had conceived and begun to realise; we mean the publishing of a treatise on docimastic chemistry. He would have inserted in it a multitude of isolated remarks and facts. Besides, such a work is entirely wanting, and nobody was better qualified to execute it.

To much general knowledge, Descotils joined a very solid judgment. If he did not perform so much as he promised, the reason was that he was threatened for more than 10 years with the cruel malady under which he sank. Under the appearance of strength and health, he suffered a perpetual uneasiness, which rendered the labours of the laboratory very disagreeable, and often insupportable. He exhibited an example of the best social and domestic virtues. He was devoted to his friends; full of affection for a father who has survived him; occupied continually with the care of his children; an excellent husband; the happiness which he enjoyed with an admirable wife was noticed by all. Descotils had a very elevated character, such as we should like to find in all those who cultivate the sciences. He bequeaths to his two sons a name which he has honoured by his labours and his noble qualities, and which will long recal the loss of a distinguished philosopher and an excellent man.

He died Dec. 6, 1815, of a chronic peritonites, at the age of 42.

Article II.

On the Chemical Phenomena of Heat. By Mr. J. B. Emmett, of Trinity College, Cambridge.

The following pages contain a brief examination of some facts relating to the effects of heat. They are the beginning of a series of papers on the principles of chemical philosophy. In some of the first communications I propose examining these subjects in a popular point of view, in order that they may be readily understood and applied by those chemists who have not acquired any considerable degree of mathematical knowledge; afterwards I shall proceed to a more rigorous solution of the various problems, and apply the principles to the phenomena of decomposition, the investigation of the laws of affinity, simple and compound; the laws of expansion of solids, liquids, and aeriform bodies; and the laws of crystallization. The subject involves many difficulties; in our progress we shall observe many facts which the principles cannot at present explain. Let not these, however, condemn the whole; let us consider them as comets, to ascertain whose orbits we are not yet furnished with sufficient data. Regular and patient investigation will clear the path, and gradually remove these difficulties. The phenomena of crystallization will present the most numerous and greatest difficulties, which will of course come under consideration in due time, and be minutely investigated. In the course of these researches I have not met with one fact which militates against the principles laid down.

All bodies, whether in the state of solidity, fluidity, or aeriform elasticity, are augmented by an increase, and contracted by a diminution, of their sensible heat. From these phenomena, it is manifest that the particles of that which we usually denominate matter are acted upon by two powerful antagonist forces—one centripetal, the other centrifugal; and when the temperature of any body is the same as that of the surrounding medium, these forces, acting between any two adjacent particles, must balance each other.

Of the nature of the centripetal force we know but little; it is probably, as I shall subsequently show, a modification of that power which causes the great bodies of the planetary system to tend towards each other. As, however, an investigation of this subject is intimately connected with the chemical agencies of electricity, I shall not examine into the nature of this centripetal force till, in a future paper, the relations of heat to light and electricity have been considered. As we know of no centripetal force which deviates from the general law, viz. force varying as $\frac{1}{\text{dist}^2}$. I shall assume, in the present paper, this as the probable law of corpuscular action, by which I mean the action of one corpuscule upon another, and not that of a system of corpuscules upon one. From the circumstance of corpuscular action in solids vanishing at inconceivably small distances, the force which preserves the aggregation of solids has been supposed to vary much more rapidly than $\frac{1}{d^2}$. This apparent deviation I shall subsequently prove arises from the mode of action arising from the order of arrangement of the corpuscles in solids.

The centripetal force is the effect of that which produces the sensations of heat, and is usually denominated calorific repulsion. Two different hypotheses have been advanced to account for the effect of heat, so far as it is concerned in the phenomena of expansion, contraction, and chemical decomposition. Of these, one appearing to me totally erroneous, and the other only defective, I shall examine into and state the arguments by which I have been induced to reject the one supported principally by Count Rumford, and choose that first proposed by Dr. Black and Lavoisier.

The one supposes that in solids the particles are in a constant state of vibratory motion, the particles of the hottest bodies moving with the greatest velocity, and through the greatest space; that in liquids and elastic fluids, besides the vibratory motion, which must be conceived greatest in the last, the particles have a motion round their own axis with different velocities, their particles of elastic fluids moving with the greatest quickness; and that in etherial substances the particles move round their own axes, and separate from each other, penetrating in right lines through space. Temperature may be conceived to depend upon the velocities of their vibrations; increase of capacity on the motion being performed in greater space, and the diminution of temperature during the conversion of solids into fluids or gases, may be explained on the idea of the loss of vibratory motion in consequence of the revolution of particles round their axes, at the moment when the body becomes fluid or aeriform, or from the loss of rapidity of vibration in consequence of the motion of the particles through greater space.

This hypothesis is founded upon an assumption which cannot be admitted for a moment; that the particles of solids are in a constant state of vibratory motion. What proof have we of this? If this be the case, the atoms or corpuscules which constitute the hardest and densest solids cannot be in contact, except when they impinge upon each other; which contact can only continue for an indefinitely small portion of time; and solidity, I affirm, cannot exist except these atoms are really in mathematical contact. The reason is obvious: at equal distances from equal and similar corpuscules, the forces will be equal; if these be not in contact, they must have perfect freedom of motion round each other, which is the property only of fluids and elastic matter. Allowing this objection to be invalid, upon what physical principles or law of nature can this motion be produced; and when produced, how is it preserved from diminution? Such a motion is more than perpetual; it has constantly to oppose an antagonist force, sometimes less than, at others equal to, and sometimes greater than itself. Upon no principle can it be shown that this motion can be preserved from diminution and final decay. It is certainly unphilosophical to assume the existence of such a motion, unless we can prove that it can be maintained under all circumstances. How do these principles explain the phenomena of the different capacities of bodies for heat, and of latent heat? It has certainly been affirmed that the immediate cause of the phenomena of heat is motion, and the laws of its communication are precisely the same as the laws of the communication of motion. No one has ever, as far as I can learn, pointed out any similarity; nor has any analogy been proved to exist between the intensity of temperature and the velocity of vibration of indefinitely small atoms of ponderable matter; or between capacity for heat, and the extent of the vibratory motion. I shall make no observations upon the convertability of this vibratory motion of a minute atom into revolution round an axis, excepting that it is impossible; requesting those who maintain its possibility to explain upon what physical principles it can take place, and show the real difference in constitution which exists between solids and liquids; and that whenever a body has arrived at a certain temperature, or, in other words, the vibrating motion has attained a certain velocity, the revolution round the axis must regularly take place; also, for which there is no provision, the difference between fluids and gases; the definite and constant temperature at which a liquid assumes the gaseous form, and the reason why this point of ebullition is less in vacuo than under atmospheric pressure. It is perhaps possible for all this to be correct; but it is repugnant to common sense; and, until it shall be established upon undeniable principles, must be inadmissible. What is stated concerning ethereal substances, or what are with greater propriety denominated imponderable agents, is certainly correct in part. When these agents possess freedom of motion, their particles, if they consist of particles, must separate from each other penetrating in right lines through space: this is so evident from numerous phenomena of the radiation of terrestrial and solar heat; from the motions of light, as deduced from observations upon the aberration of the fixed stars, &c., that there is no occasion to attempt to give further proof. There appears no necessity for the motion of these particles round an axis; which motion does not explain one fact which may not be established without it. These ideas have gained considerable strength from an experiment made by Count Rumford, in which a piece of metal was kept hot for a great length of time by friction without any apparent diminution; from this it was concluded that the heat could be kept up for an unlimited time, and therefore cannot depend upon the presence of any thing material; it was in consequence supposed that the heat was the effect of a vibratory motion communicated to the particles of the metal by the friction. This inference is not just: the experiment was continued for a long time, during which time the metal was suffering diminution in consequence of the friction, and the parts immediately below the surface becoming more and more compressed. Now if we suppose every solid to contain, in the interstices between its particles, a large quantity of caloric, and that when these particles are brought into closer contact, part of this is separated, there must be a constant evolution of heat, so long as any part of the metal remains. Were there no other objections to this mode of accounting for the effects of heat, the phenomena of its radiation would be sufficient to overturn the hypothesis. Radiation takes place in vacuo; how can the vibratory motion be, in this case, produced in a body at a distance? Some argue that the best vacuum is imperfect. Certainly it is: but air at all times conducts heat very slowly from one body to another, and cannot easily conduct it downwards, yet radiation takes place in every direction; if the radiation depended upon the conducting power of the air, this effect must almost cease when the rarity of the air is indefinitely great, which is not the case. As, however, in consequence of the imperfection of the best vacuum, the motion of the particles of one body may be supposed to communicate motion to those of another at a distance, it will be proper to bring forward another example of radiation, to which this objection cannot be made, viz. that of solar heat. Heat is communicated from the sun to the earth; and we are certain that there is no conducting medium between them by which it can be transferred to so great a distance.

The hypothesis advanced by Dr. Black is totally different to that which has thus been briefly examined; is more simple and rational; but in many respects very defective. I shall now proceed to investigate it, and show how the principal facts of chemical philosophy may by it be explained.

This supposes that there is a peculiar matter of heat, consisting of particles mutually repellent, but attracted by every species of ponderable matter, capable of insinuating itself into the interstices between the particles of every body, and that the sensations of heat are produced by the motion of this matter of heat or caloric; thus a body will feel hot which imparts a portion of this fluid to the hand, and cold if it abstract it. Upon this principle may most of the phenomena of heat be explained. All those chemists whose writings upon the subject I have seen have made very erroneous statements respecting it, which certainly lead to conclusions which must overturn the system, when investigated, unless properly explained; and those who have objected to the hypothesis have principally attacked facts little connected with the subject, such as the addition of heat not increasing the weight of a body in any sensible degree, whilst real errors in principle have been entirely overlooked. Only one of these will be examined at present. Caloric is supposed to be attracted by the particles of ponderable matter surrounding each, or an atmosphere which decreases in density as we recede from the centre of the particle; and that calorific repulsion is in proportion to the density. In solids the centripetal force is supposed to exceed this repulsive power, otherwise the particles submitted to the operation of these forces could not be held together; and yet, to account for the phenomena of expansion and contraction, these particles are assumed not to be in contact with each other. Now, if the centripetal be the predominating force, how can these particles be kept asunder? The absurdity must be evident to every one who gives the subject one moment's consideration. Caloric certainly is attracted by the particles of ponderable matter; for if a bar of metal be heated at one end, the other soon becomes hot; this heat can only be conducted in consequence of its being successively attracted by the different particles which compose the bar.

Of the form of the corpuscules, atoms, or particles of matter, we have no certain knowledge. They are most probably spheres. They may with safety be assumed of a spherical form; as that figure, of all others, is the most simple, and will answer every condition required, as I shall subsequently demonstrate. They have been assumed by some of a spheroidal form, in consequence of the forms of some crystals, and the supposed harmony which exists, if this be the shape, between the figure of the ultimate atoms of matter and the planetary bodies. For this there is no necessity, since we are assured that this figure of the planets arises entirely from their diurnal rotation, and could not be produced unless they consisted of an indefinite number of minute particles of matter, which cannot be the case with the particles under consideration; therefore the spherical, and not the spheroidal figure, is in harmony with that of the planetary bodies. However, as we have no evidence to the contrary, I shall assume all bodies in nature to be composed of minute spherical atoms.

Since caloric is attracted by the particles of matter, it must surround each as an atmosphere, whose density diminishes more rapidly than the intensity of the attraction, as we recede from the centre of any particle. That this law really attains admits of easy demonstration.

Suppose C (Plate LXVII., Fig. 1.) the centre of a minute particle of matter, C *n* a radius produced; at N draw P N $\perp$ C N, and suppose P N to represent the centripetal force at the surface; describe P P′ Q, a curve such that every ordinate P′ *n* shall measure the centripetal force at the distance C *n*; let C . *n* = *x* and P N = *y*; in this curve $y \alpha \frac{1}{x^n}$ (*n* being most probably = 2); draw any other curve, *p* P′ L, such that $y \alpha \frac{1}{x^{n-m}}$, $m < n$; let *p* N < P N, the curves will intersect each other in some point P′; at *n* ∴ the forces are =, and consequently another = and similar corpuscule placed with its centre at *n* will be in = librio; let (Fig. 2) be increased (increasing the ordinates of the curve *p* P′ L representing an augmentation of the calorific repulsion arising from increase of temperature) P′ will approach nearer P, or contraction arise from elevation of temperature, which is absurd; when ∴ *p* N is very small, *n*, the point of = librio will be far removed from N, and as the heat is increased, it will approach to C, or a mass composed of such particles gradually contract, till *p* N = P N, after which the most minute movement of heat must separate the corpuscles *ad infin.*, as the curves never can in that case intersect each other, which is altogether contrary to the observed order of things. Nearly similar is the result of $y \alpha \frac{1}{x^n}$. If, however, the centrifugal force $\alpha \frac{1}{x^{n+m}}$ increments of heat produce continual separation between two, and consequently expansion of an aggregate of a great number of particles, which accords with every fact. We are totally unacquainted with the manner in which caloric is attracted by these particles. However, I am inclined to believe that the centripetal force exerted by any corpuscle upon caloric α $\frac{\text{Quantity of matter in the corpuscle}}{\text{distance})^2}$: the facts from which this inference is drawn will be considered in a future paper. I shall now proceed, in general terms, to find measures for the temperature, capacities for heat, &c.

Let C represent the centre of a corpuscle, C N *n* a radius produced. Draw *r s* $\perp$ N *n* to represent the density of caloric, or temperature of the ambient medium. Draw *s o* // C *r* and P *p t*, the curve representing the density of the calorific atmosphere corresponding to the temperature *r s*. Draw another curve P′ N C *p*′ P′ such that its ordinates P′ N, &c. = P N × dist.^{2} from C. The area of this curve will represent the quantity of caloric surrounding the atom; *r s* is that part which alone affects the thermometer; the

Fig. 1.

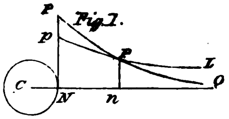


Fig. 2.

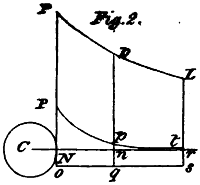


Fig. 3.

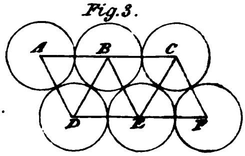


Fig. 4.

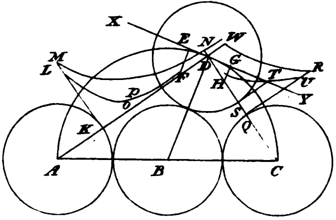


Fig. 5.

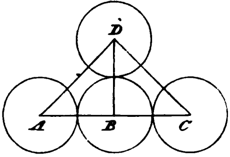


Fig. 6.

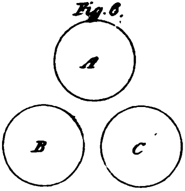


Fig. 7.

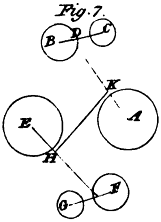


Fig. 8.

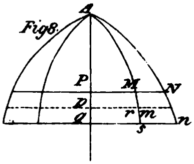


Fig. 9.

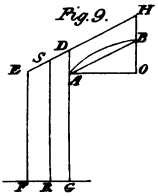


Fig. 10.

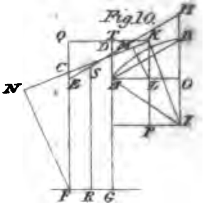


Fig. 11.

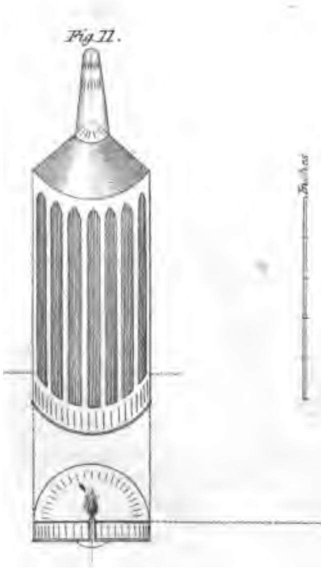
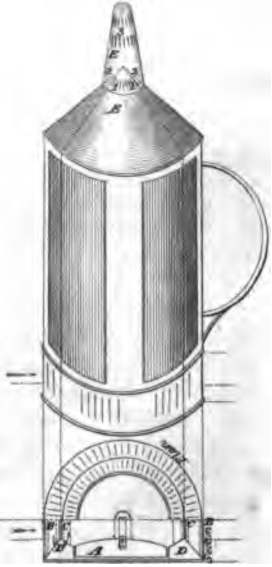


Fig. 12.



area P′ N C p' P′ represents the latent heat. By latent heat nothing more is meant than the heat which is attracted by a particle or number of particles, and which is $\therefore$ not communicated to any other body, except of a lower temperature; the increment of the area P′ N C p' P′ is the capacity corresponding to any change of temperature; $p\,q - n\,q$, or the excess of density of the caloric at any distance, above its density $n\,q = r\,s$ in the ambient medium, is a measure of the calorific repulsion, or the tendency which the caloric will have to produce expansion. If the centripetal force really $\propto \frac{1}{\text{dist}^2}$, then the distances from the surface of a particle of matter being in harmonial progression, this will be in geometric. The centripetal force $\propto \frac{1}{\text{dist}^n}$, and the centrifugal $\propto \frac{1}{\text{dist}\ n + m}$, it immediately follows that the particles which constitute solids are in actual contact with each other, and that the phenomena of expansion and contraction arise from an alteration which takes place in their arrangement. Let A B C D E F (Fig. 3) represent a number of spherical particles mutually attracting each other. They will arrange themselves in such order that straight lines joining their centres shall form equilateral triangles; for then the sum of their mutual actions upon each other will be a maximum. The introduction of any repulsive power, whose intensity diminishes more rapidly than that of the centripetal force, will produce an alteration in the arrangement of these particles, which occasions an expansion of the mass. Let A B C D (Fig. 4) represent four = and similar particles of matter; with centre B and radius B C describe the semicircle A D C, the sphere D may evidently roll upon B, and the locus of this centre will always be in the semicircle, and its initial motion will always be in the direction of a tangent at the point D. Join A D, B D, C D, and draw $x\,y$ a tangent at D; at Q erect the $\perp$ R Q to represent the centripetal, and Q U the centrifugal force at the surface of O; draw the curves R W, U T G, whose ordinates represent the intensity of the forces at all distances; draw the same curves belonging to A; in solids R Q = M K > U Q = L K $\therefore$ the particles must be in contact. W D = centripetal force, which tends, at the distance D C, to bring D and C in contact; and S T represents the force which tends to separate them; N D is that force which tends to bring D and A into contact, and p O that which has a tendency to separate them; the differences between these forces are the powers which tend to put the particle D into motion. Represent W D − S T by D H, and N D − p O by D F, and at H and Y draw H G, F E $\perp$ the tangent. Then by the resolution of forces, E D and D G are the two which keep the particle D in equilibrio, or tend to put it in motion E F, G H $/\!/$ D B preserve the contact with B; when the particle D is stationary, *i. e.* the body has attained the temperature of the surrounding medium E D = D G; $\therefore$ if D were very near C when the temperature was first raised, D G would be $<$ E D $\therefore$ the particle must move on towards A, till E D $=$ D G, then its expansion is a maximum. Since in every mass of matter the particles must be arranged in rows running in all possible directions, the whole volume must expand. Did the rows all extend in one direction only, the expansion could take place in the direction $\perp$ to it.

We may now see clearly whence arises the difference in the capacities of solids for heat. The first cause is the different attractions of solid particles for heat; a particle which attracts heat more powerfully than another will, *cæt. par.* be surrounded with a more dense calorific atmosphere, and will consequently require a larger quantity of the matter of heat to raise it from any one to another given temperature; $\therefore$ if the interstices in solids were $=$, there would be a difference of capacity; the second is the distance of the particles from each other, on which the altitude of this atmosphere depends; the next cause is the situation of the particles with respect to each other; as the temperature of a solid is elevated, the interstices become enlarged $\therefore$ the calorific atmosphere becomes more and more extensive, and when the centrifugal force $>$ centripetal, a separation takes place; consequently that part of the atmosphere which is principally enlarged is on and near the surface of the particle where it is densest $\therefore$ during separation a large quantity of heat must disappear; hence all the capacity of every solid increases as the temperature is increased.

From what has been stated respecting solids, it is evident that the capacities of $=$ weights of different substances do not represent the real capacities; for as the ratio of the capacities is that of one atom of each substance, the capacities ought to be calculated for weights in the ratio of the weights of the ultimate atoms: this will appear more clearly when the capacities of fluids, gases, and compound substances, are examined.

When the temperature of a solid is elevated to a certain degree, D B becomes $\perp$ A C (Fig. 5), for D F $=$ D H $\therefore$ the solid just begins to lose its cohesion. The most minute addition to the sensible heat causes an entire separation of the particles, as A B C (Fig. 6); and the particles, being now capable of motion in any direction, must constitute a fluid.

Their surfaces now not being in contact, the quantity of caloric in each atmosphere has to be increased in that part which is most dense; hence during the conversion of a solid into a fluid, much caloric disappears. Were it not for atmospheric pressure, bodies would, evidently, only exist in the state of solids and permanently elastic fluids; for when the particles are separated, they attract caloric more powerfully than they do each other $\therefore$ above that temperature which fuses a solid, its particles, by attracting caloric, and continually increasing the extent of their calorific atmospheres must separate *ad inf.* without becoming previously fluid. This is proved to be correct by many experiments. Sal-ammoniac, oxide of arsenic, and many other substances, at a certain temperature, volatilize without becoming previously fluid; yet, under strong atmospheric pressure, they are easily fusible. Also water, confined under the receiver of a powerful air-pump, exposed to an extensive surface of concentrated sulphuric acid, is congealed; the vapour, which is continually emitted by reason of the vacuum, which is preserved nearly perfect by the sulphuric acid, robs the residuary water of so much of its caloric as to congeal it, and the ice itself evaporates without undergoing liquefaction. Here we see the capacity of this exceedingly rare vapour is enormous; it is entirely owing to this that it is able, in the first instance, to congeal the water. We must next ascertain the manner in which the atmospheric pressure preserves bodies in a state of fluidity.

Under the pressure of an atmosphere, a liquid cannot be made to boil till its particles are separated to such a distance that the particles of which the atmosphere is composed can enter into the interstices: the repulsive force so far exceeds the centripetal that it can overcome this atmospheric pressure. Hence we should be led to expect all liquids to have their point of ebullition depressed an equal number of degrees when in vacuo: this appears, from the experiments of the late Dr. Robinson, to be the fact. Hence we see that the pressure being invariable, the point of ebullition of a liquid is constant; also we perceive, on account of atmospheric pressure, the sudden and remarkable change which bodies undergo during their conversion into aeriform fluids; also the immense quantity of caloric which at this moment becomes latent; the particles which were previously removed from each other only a portion of the diameter of any single particle, are now separated 10, 12, or more diameters; and though the calorific atmosphere at this distance is very rare, its great extent produces the change of capacity which the body undergoes during this change of form. Gases, under ordinary circumstances, are subjected to peculiar laws; collected as usual, they all possess the same degree of elastic force, *i. e.* the excess of the centrifugal above the centripetal force, is constant, = generally to 29 inches of mercury; hence the specific gravity of any gas depends upon the real weight of an atom, the attraction of its particles for each other and for caloric. Here if we assume the centripetal force to vary $\frac{1}{d^2}$, and take the common expression of the variation of the density of an atmosphere surrounding a sphere, $h \times h y \log. \frac{1}{v} = r - \frac{r^2}{x}$ (Dealtry's Flux.), we shall find a ratio between the increase of temperature and the expansion of aeriform matter, which appears from experiment to be the real law; the investigation of this point I at present omit, deferring it till the whole principles, here briefly explained, come under a more strict mathematical examination.

When two or more particles of a different kind attract each other, they will be acted upon in the same manner as two similar atoms; this is too evident to require demonstration. The difference between a chemical union and mechanical mixture is this; the particles of which each species of matter concerned is composed attract those of the other rather than its own ∴ two or more dissimilar particles form a system, the union between them being more intense than that force which binds together the aggregate. Suppose A, B, C, (Fig. 7) to be three dissimilar particles in chemical combination, B and C are in = librio, *i. e.* the forces which act upon them balance each other; if they have freedom of motion, they may be made to move round their centre of gravity; let them combine with a third particle, A; the whole must now be in = librio; and on placing together any number of such systems, their centres of gravity H, K, will be in = librio.

Caloric acts in two ways upon compounds: it tends to separate the centres of gravity of the systems, and also destroy the union between the individual particles of which these systems are composed; for since the forces act upon all the particles, an increase of heat must increase their distance from each other. Experiments which prove the fact are numerous: alcohol boils at a moderate temperature, distilling without alteration: expose it to a red heat by passing it through an ignited tube, and it is decomposed. Caloric acts in several apparently opposite ways upon compounds. In some cases it promotes, and in others destroys chemical union. When it promotes chemical union, the attraction of the heterogeneous particles is not sufficient to overcome their attraction for each other; at the ordinary temperature of the atmosphere, an elevation of temperature sometimes lessens one and sometimes the other of these attractions, according to the altitude and density of the calorific atmospheres, so as at one time to cause bodies to combine, at another to destroy their union. For example, the particles of copper have for each other so strong an attraction that their attraction for oxygen, and that of sulphuric acid for the oxide of copper, cannot dissolve or in any way act upon the copper in the cold; when the acid is elevated to its boiling point, it is partly decomposed, the particles of the copper are oxygenated, and the oxide dissolved; when the substance thus formed is ignited, it is again decomposed, the acid is expelled, and a part of the copper seems to be reduced to its metallic state. From this it is manifest that the order of affinity must always depend in part upon temperature; and the affinities, or rather attractions of different bodies for each other, evidently depend primarily upon the quantities required for saturation; yet temperature, insolubility, elasticity, specific gravity, and electricity, have so much influence that the real law is with difficulty ascertained.

(*To be continued.*)

ARTICLE III.

Researches respecting the Laws of the Dilatation of Liquids at all Temperatures. By M. Biot.

(*Concluded from* p. 377.)

We can compare our results advantageously with those of Blagden and Gilpin, because their alcohol was weighed at temperatures much inferior to the boiling point of alcohol. To do this we must calculate the values of the true dilatation δ_T for different temperatures included in their experiments. This may be done by means of the formula

$$\delta_T = K\,T + \frac{D}{80}\{A\,T + B\,T^2 + C\,T^3\}\{1 + K\,T\}$$

We thus obtain the following comparisons:—

Degrees of the mercurial thermometer.	Ditto of the alcohol therm. calculated D_T	True dilatation from the freezing point δ_T		
		Calculated.	Observed.	Difference.
32° F or 0 R	0·000	0·00000	0·00000	0·00000
50 …… 8	6·409	0·01008	0·01000	−0·00008
70 …… 16·89	13·939	0·02191	0·02175	−0·00016
95 …… 28	23·753	0·03733	0·03737	+0·00004
100 …… 30·22	25·808	0·04056	0·04053	−0·00003

The agreement of this result is surely as perfect as could be hoped for; and the deviations may just as well be ascribed to the experiments as to the calculation. In reducing the coefficients of δ_T and Δ_T into numbers, we shall have the following results for any temperature T expressed in degrees of Reaumur's thermometer.

Degrees of the alcohol thermometer on its own scale:—

$$D_T = 0{\cdot}784\,T + 0{\cdot}00208\,T^2 + 0{\cdot}00000775\,T^3$$

Apparent dilatation from 0 in glass vessels:—

$$\Delta_T = 0{\cdot}00120085\,T + 0{\cdot}00000318593\,T^2 + 0{\cdot}00000001187\,T^3$$

True dilatation:—

$$\delta_T = 0{\cdot}00123369\,T + 0{\cdot}00000322537\,T^2 + 0{\cdot}00000001198\,T^3$$

In the true dilatation I suppress the term containing T^4, the coefficient of which is 4 preceded by 12 zeros before the decimal point. It is evident that an error only amounting to $\frac{8}{1000000}$ on the whole dilatation to 80° would not enable us to introduce this term. We must not forget that these formulas apply only to very strong alcohol; for we have seen that the dilatation of this liquid follows a different law when it contains a great proportion of water. By means of these formulas we may employ indifferently either mercurial or alcohol thermometers. We may likewise employ them to reduce to the same temperature weights taken in alcohol.

I shall now make the same calculations for water. By comparing the weights of the same volume of water observed by Gilpin and Blagden at 35°, 40°, and 45° of Fahrenheit, I deduce by interpolation the weight of the same volume at 32° Fahrenheit which corresponds with 0 of our scale. Then comparing this result with the weights observed by the same philosophers at 40°, 50°, 70°, 95°, and 100°, I deduced the relation of the volumes at these temperatures, considering the primitive volume at 32° as unity. Thus I obtained the following results:—

Degrees of the mercurial thermometer.	True volume of water observed.	True dilatation of ditto from the freezing point δ_T
32 F or 0 R	1·00000	0·00000
40 ...... 3·56	0·99988	−0·00012
50 ...... 8·00	1·00014	+0·00014
70 ...... 16·89	1·00188	+0·00188
95 ...... 28·00	1·00583	+0·00583
100 ...... 32·22	1·00684	+0·00684

To deduce from these results the dilatation D from 0 to 80 Reaumur, I shall employ the last two observations as I did for alcohol. I shall consider them as given values of δ_T; and since we can calculate D_T for the same temperatures from the water thermometer, we shall deduce D from the formula

$$\delta_T = KT + \frac{D\{1 + KT\}}{80} D_T$$

We find in the first place

$$T = 28{\cdot}000;\ D_T = 8{\cdot}9264;\ KT = 0{\cdot}000919$$
$$T = 32{\cdot}222;\ D_T = 10{\cdot}6818;\ KT = 0{\cdot}000902$$

From observation we have

$$T = 28{\cdot}000;\ \delta_T = 0{\cdot}005829;\ \delta_T - KT = 0{\cdot}004910;\ \frac{\delta_T - KT}{1 + KT} = 0{\cdot}004050$$

$$T = 32{\cdot}222;\ \delta_T = 0{\cdot}0068409;\ \delta_T - KT = 0{\cdot}0058485;\ \frac{\delta_T - KT}{1 + KT} = 0{\cdot}0058427$$

By substituting these values in the formula we obtain the two following equations:—

$$0{\cdot}004905 = \frac{8{\cdot}9264}{80} D;\ 0{\cdot}0058427 = \frac{10{\cdot}6818}{80} D$$

We see here that the smallness of D_T renders the determination of D much less favourable than in the case of alcohol. It would have been much more advantageous if we could have employed experiments made at higher temperatures. But the extreme care of the experimenters in a great measure compensates for this disadvantage; for the two values of D obtained from these equations agree very well with each other. The first gives

$$D = 0{\cdot}0439595$$

The second—

$$D = 0{\cdot}043859$$

This is the apparent dilatation of water in glass from 0 to 80°. To obtain the true dilatation within these limits, we must employ the formula

$$\delta_{80} = 80\,K + \{1 + 80\,K\}\,D$$

Substituting for K and D their values, we get

$$\delta_{80} = 0{\cdot}046601$$

This is the true dilatation from 0 to 80° Reaumur.

The only experiments with which I am acquainted to which we can compare this result are those of Nollet. This philosopher says that common water in a graduated tube of glass, when heated from the freezing point to the boiling point, expands a little more than $\frac{37}{1000}$ of the volume which it occupied at the first of these temperatures; and he adds that it acquires this dilatation in a minute and some seconds. The apparent dilatation which we have found is greater than that of Nollet by 0·006, or $\frac{8}{1000}$ of the primitive volume. But, from the short time that Nollet kept the graduated tube in boiling water, it is very probable that it did not quite reach the boiling temperature; and, besides, the escape of vapour ought to have prevented it from acquiring as much heat as it would have done in a close tube like the thermometers of Deluc, according to which we have regulated our formulas. A proof that some cause of this nature influenced the observations of Nollet is, that he gives also the apparent dilatation of mercury in his instrument, and finds it amount to $\frac{14}{1000}$ from the temperature of freezing water to the boiling temperature of the same liquid. But from the very exact experiments of Lavoisier and Laplace it amounts to $\frac{158}{10000}$. Admitting our value of D as quite exact, the temperature of the water in Nollet's tube would have been 74° R., instead of 80°, when he observed the dilatation of water; and, according to the experiments of Lavoisier and Laplace, it would have been 71° when he observed the dilatation of mercury. Perhaps, likewise, there was some inaccuracy in the graduation of his instrument.*

We can with more certainty compare our formula with the experiments of Blagden and Gilpin. For this purpose we must calculate the values of the true dilatation δ_T for the temperatures which we wish to take as an example. This may be done by means of the formula

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* Our result is equally confirmed by the experiments of Dalton, as will be seen hereafter.

$$\delta_T = K\,T + \frac{D\{1 + K\,T\}}{80}\,D_T$$

We obtain from it the following comparisons, in which unity of volume is the volume of water at 0.

Degrees of the mercurial thermometer.	Ditto of the water therm. calculated D_T	Real dilatation from the temperature of freezing water.		
		Calculated.	Observed.	Difference.
32 F or 0·00 R	0·00	0·00000	0·00000	0·00000
40 ...... 3·56	− 0·3373	−0·00007	−0·00012	+0·00005
50 ...... 8·00	− 0·1220	+0·00019	+0·00014	−0·00005
70 ......16·89	+ 2·3840	+0·00184	+0·00188	+0·00004
95 ......28·00	+ 8·9264	+0·00581	+0·00583	+0·00002
100 ......30·22	+10·6818	+0·00685	+0·00684	−0·00001

We see that the formula is as exact as the observations themselves. The differences never occur but in the hundred thousandths. Thus these experiments which required so much delicacy, as the authors of them testify, might have been ascertained by calculation, as we have done from the thermometric observations of Deluc combined with a single measure of the absolute dilatation of water. If we reduce the coefficients of δ_T into numbers, we obtain the following results for any temperature T expressed in degrees of Reaumur:—

Degrees of the water thermometer on its own scale:—

$$D_T = -\,0{\cdot}16\,T + 0{\cdot}0185\,T^2 - 0{\cdot}00005\,T^3$$

Apparent dilatation of water in glass:—

$$\Delta_T = -0{\cdot}000087718\,T + 0{\cdot}0000101424\,T^2 - 0{\cdot}000000027412\,T^3$$

True dilatation:—

$$\delta_T = -\,0{\cdot}000054878\,T + \text{[illegible]}0000101395\,T^2 - 0{\cdot}000000027080\,T^3$$

I neglect the term T^4, which is equal to 9 preceded by 12 zeros all decimal. It would only amount to $\frac{4}{100000}$ of the primitive volume even at 80°. We must not forget that the law of the dilatation changes when other substances are dissolved in water.

The value of δ_T is susceptible of a minimum, which will give us the absolute condensation of pure water. The equation which determines this minimum is

$$0 = -\,0{\cdot}000054878 + 0{\cdot}000020279\,T - 0{\cdot}00000008124\,T^2$$

If we resolve this equation in the usual manner, and take only the smallest root, we obtain T = 2·736° Reaumur, or 38·16° Fahr. Gilpin and Blagden, according to Dr. Thomson, placed the true maximum at 39°; and Dr. Hope, from the motion of water in vessels furnished with thermometers, placed it at 38° Fahr. Some small variation may exist in this point in consequence of differences in the thermometers employed in the experiments, and likewise of the greater or lesser purity of the water examined; for we have seen that the presence of foreign bodies in that liquid sinks the point of its greatest condensation, and even makes it entirely disappear; but our calculation, which approaches the mean of the experiments, leaves but a very small range to the true point.

In a curious set of experiments made by Sir Charles Blagden to determine how far water in certain circumstances could be cooled down without freezing, he observed that its retrograde dilatation continued, and proceeded with such rapidity as to form a considerable proportion of the total expansion which water undergoes when converted into ice. This is an evident consequence of our formulas. In the value of the apparent dilatation Δ_T, when T is positive, a part of the terms destroy each other by the opposition of their signs; but below 0°, T becoming negative, all the terms take the same sign, and must be added to each other. To know how far the difference can go, let us calculate the value of Δ_T at + 10° R. and − 10° R. We obtain

$$T = + 10^\circ \qquad \Delta_T = 0{\cdot}0001097$$
$$T = - 10^\circ \qquad \Delta_T = 0{\cdot}0019188$$

We see that the second is 18 times greater than the first.

Knowing the value of the true dilatation δ_T, it is easy to deduce from it the apparent dilatation in vessels of any kind whatever; for calling the cubic dilatation of the vessel K, the apparent dilatation Δ_T is given generally by the equation

$$\Delta_T = \frac{\delta - K\,T}{1 + K\,T}$$

If we wish only to consider the dilatation Δ_T for low temperatures, when it will always be small, we may neglect the product of $\delta_T - K\,T$ by $K\,T$, and suppose the denominator of the second member equal to unity. Then we have simply

$$\Delta_T = \delta_T - K\,T$$

We shall use it in this state for the purposes to which we mean to apply it. For greater simplicity, we shall substitute the letters a, b, c, for the numerical coefficients contained in δ_T; that is to say, we shall take in general

$$\delta_T = a\,T + b\,T^2 + c\,T^3$$

a, b, and c, having the values which we have just determined. Substituting this expression in Δ_T, it becomes

$$\Delta_T = (a - K)\,T + b\,T^2 + c\,T^3$$

The apparent dilatation Δ_T may be susceptible of a minimum, and the temperature at which it happens will depend upon the dilatability of the vessel. The equation which determines this minimum is

$$\frac{d\Delta_T}{dT} = 0; \text{ or } 0 = a - K + 2bT + 3cT^2$$

a quadratic equation, the roots of which are

$$T' = \frac{-b + \sqrt{b^2 - 3(a - K)c}}{3c};\ T'' = \frac{-b - \sqrt{b^2 - 3(a - K)c}}{3c}.$$

These two roots will be both positive when the vessel is of a nature to dilate itself by heat; for since a is negative as well as c, the product $3(a - K)c$ will in that case be positive. The value of the radicle will be then less than b, and as the denominator $3c$ is negative, the two roots will have the sign +. But the first is the only one which interests us; for it is the only one which is always very small. To calculate it exactly, and with facility, we must make c, disappear from the denominator by multiplying the two terms of the fraction by

$$b + \sqrt{b^2 - 3(a - K)c}$$

This gives us

$$T'' = -\frac{(a - K)}{b + \sqrt{b^2 - 3(a - K)c}}$$

Nothing remains but to substitute for K its value in this formula, and we obtain the temperature T of the apparent maximum of condensation. The absolute maximum of condensation will be found by making $K = 0$. This gives

$$(T) = -\frac{a}{b + \sqrt{b^2 - 3ac}}$$

It was in this manner that we calculated in a preceding part of this paper. As the value of c is very small, if the temperature T of the maximum be low, we may obtain a near approximation, though we neglect the term $3cT^2$ in the equation which determines this maximum, and then we obtain

The apparent maximum $T = -\frac{a}{2b} + \frac{K}{2b}$

The true maximum $(T) = -\frac{a}{2b}$

This gives us

$$T = (T) + \frac{K}{2b}$$

This result shows us how the apparent maximum depends upon the true maximum and upon the dilatation of the vessel. It shows us that, in order to obtain the temperature T of this maximum, we must necessarily have regard to the term which contains the square of the temperature in the law of the dilatation of water. But this simple result is only an approximation. The true expression is

$$T = -\frac{(a - K)}{b + \sqrt{b^2 - 3(a - K)c}}$$

which may sensibly differ from the approximate one, when the vessels are very dilatable; for in that case the values of T and K become greater, and the error committed by neglecting $3\ c\ T^2$ more sensible.

I shall apply this result to the experiments made by Mr. Dalton on the apparent maximum condensation of water in dilatable vessels. The experiments are as follows.* I have joined to them the cubic dilatation of the vessels employed:—

Vessels.	Cubic dilatation of ditto for 1° Reaumur.	Max. condensation observed in degrees of Reaumur.	Degrees at which water is equally dilated.
Flint glass......	0·00003003	4·222°	0 and 8·444°
Iron	0·00004578	4·667	9·334
Copper	0·00006309	6·000	12·000
Brass	0·00007002	6·222	12·444
Pewter	0·00007266	6·664	13·328
Lead	0·00010689	7·778	15·555

The following table exhibits a comparison of these results with our formula:—

Vessels.	Apparent maximum in degrees of Reaumur.		
	Calculated.	Observed.	Difference.
Flint glass	4·236	4·222	−0·014
Iron	5·072	4·667	−0·405
Copper ..	5·960	6·000	+0·040
Brass ...	6·319	6·222	−0·097
Pewter ..	6·456	6·664	+0·108
Lead ...	8·246	7·778	−0·468

The differences existing between calculation and experiment are very slight. They may depend upon some slight difference between the dilatability of the substances examined by Lavoisier and Laplace, and those employed by Mr. Dalton in his experiments. This is the more likely, because errors in the dilatation are very much increased by the smallness of the divisor with which they are affected in the expression of T; but as the slight deviations are generally negative, I am disposed to suspect that the water employed by Mr. Dalton, at least in some of his experiments, was not quite pure, but contained a small quantity of salt, which sank a little its maximum of condensation. This explains why, in making use of vessels the dilatation of which was almost insensible as stone ware, Mr. Dalton found the apparent maximum lower than the ordinary term of the true maximum once, among other examples, at 1·78° R., while our formula for pure distilled water gives the true maximum at 2·74° R., nearly 1° higher than the observation of Dalton.

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* Nicholson's Journal, x. 93.

Mr. Dalton observed, likewise, that in his vessels the water stood at the same height by equal changes of temperature above and below that which corresponded to the apparent maximum of condensation. This is another consequence of our formula. The general expression of the apparent dilatation Δ_T in these low temperatures is

$$\Delta_T = (a - K)\,T + b\,T^2 + c\,T^3$$

and calling T' the temperature of the apparent maximum of condensation, we have seen that this temperature was given by the equation

$$0 = a - K + 2\,b\,T' + 3\,c\,T'^2$$

Let us make in general

$$T = T' + t$$

that is to say, let us reckon the temperatures above and below the apparent maximum of condensation. Substituting this value of T in Δ_T, we shall have

$$\begin{array}{rllll} \Delta_T = & (a - K)\,T' & + \;\; b\,T'^2 & + \;\; c\,T'^3 & \\ + & (a - K)\,t & + 2\,b\,T'\,t & + 3\,c\,T'^2\,t & \\ & & + \;\; b\,t^2 & + 3\,c\,T'\,t^2 & \\ & & & + \;\; c\,t^3 & \end{array}$$

The first line is constant: it is the value of the dilatation Δ_T at the epoch of the maximum of condensation. We will represent it by $\Delta_{T'}$. The second line is all multiplied by the first power of t. When we unite all its terms, the factor of t is $a - K + 2\,b\,T' + 3\,c\,T'^2$, and this factor is null, because T' is determined precisely so as to render it null. Hence the whole expression of Δ_T becomes

$$\Delta_T = \Delta_{T'} + \{b + 3\,c\,T'\}\,t^2 + c\,t^3$$

We have seen that the coefficient c is very small; for we have $c = -\,0{\cdot}00000002708$. Hence if we extend the comparison of heights to 20° Reaumur on both sides the maximum, we shall have $t = \pm 20$, and there result $c\,t^3 = \mp\,0{\cdot}0002166$; that is to say, that this term would not alter the statement more or less than $\frac{2}{10000}$ of the whole volume of the water at 0. If we take $t = 10$, the effect will be eight times less; so that unless the experiments be almost mathematically exact, the effect of this term will not be perceived; but if we neglect it, the value of Δ_T will be reduced to

$$\Delta_T = \Delta_{T'} + \{b + 3\,c\,T'\}\,t^2$$

then it remains the same when t has equal values either positive or negative. Now this is the property observed by Mr. Dalton.

The same philosopher has observed, likewise, the quantity that water sinks suddenly in vessels of different kinds when plunged into a hot liquid. He found this quantity to vary with the vessel, and to increase with the dilatability of the vessel.

Hence he properly concluded that this subsidence was owing to the dilatation of the vessel, which, conducting heat better than water, is heated sooner, and of course begins to dilate first. What proves this still better is, that the amount of this subsidence is nearly proportional to the cubic dilatation of the substances of which the vessels are composed. The pewter vessel alone seems to constitute an exception, because the subsidence indicated is a little less than that of brass, whereas it ought to be a little greater. But if this be not a typographical error, it may be owing to the great difficulty of making such delicate observations, and of measuring the sudden subsidence of the water before it has acquired any sensible increase of heat.*

I shall finish these researches on the dilatation of liquids by pointing out a process, which results from them, and which appears to me both simple and exact, for measuring the different dilatations of solid bodies. It consists in observing the apparent dilatation of a liquid; for example, mercury, in vessels composed of the substances which we wish to try, and to observe always between two constant temperatures, as, for example, 0° and 80° Reaumur. This apparent dilatation may be observed with facility as accurately as we please. When known for one species of vessel of which K is the cubic dilatation, we shall have between the true and apparent dilatations δ_T, Δ_T, the equation

$$\Delta_T \{1 + K\,T\} = \delta_T - K\,T$$

which gives

$$\{1 + \Delta_T\}\,T\,K = \delta_T - \Delta_T$$

For another kind of vessel subjected to the same temperatures, we shall have, in the same manner,

$$\{1 + \Delta_{T'}\}\,T\,K' = \delta_T - \Delta_{T'},$$

δ_T will remain the same, because the same liquid was employed. Subtracting these equations from each other, this quantity disappears, and there remains

$$\{1 + \Delta_{T'}\}\,T\,K' - \{1 + \Delta_T\}\,T\,K = \Delta_T - \Delta_T$$

from which we obtain

$$K' = K + \frac{\{\Delta_T - \Delta_T'\}\{1 + K\,T\}}{T\{1 + \Delta_T'\}}$$

In the present state of science, the dilatation of the metals is known sufficiently to enable us to employ them to calculate the small correction dependant on K in the second member of the preceding equation. Then substituting for Δ_T, $\Delta_{T'}$ and T, their observed values, we shall find K′ — K. The accuracy of these values will be so much the greater as Δ_T and $\Delta_{T'}$ are observed in volumes, and as it is the difference of the cubic dilatations K′ — K which is given by this equation. Perhaps in experimenting on the most dilatable metals, it would be necessary to preserve the term proportional to $K^2 T^2$, which we neglected at the beginning of this memoir. Experiment only can show if it becomes sensible.

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* M. Biot does not appear to know that *pewter* is not *tin*, but an alloy of tin with another metal. Its expansion is less than that of brass, but greater than that of tin. T.

To have the absolute values of K and K′ by the same process, we must know the true dilatation δ_T of the liquid which we employ. We will obtain it by observing this dilatation in a vessel which is not dilatable; and it is easy to construct one possessed of this property, namely, which compensates itself, when we know the difference of the dilatation of the metals.

I conceive that this process will afford an exact and simple method of comparing the dilatation of mercury with that of the metals; which is the only thing that remains to be done to enable us to bring all the dilatations to the air thermometer, which is the most perfect of all; for the absolute dilatations of the metals appear perfectly known from the experiments of Lavoisier and Laplace, to the exactness of which it is difficult to conceive that any thing can be added.

If we adopt them, they will furnish us with the means of calculating the true dilatations of the liquids when we know their apparent dilatation in vessels of known dilatability. To find the law of these last relative to a given liquid, we must begin by constructing a thermometer with it, and hermetically sealing it. It must then be compared with the mercurial thermometer. The coefficients A, B, and C, will be determined by three of these observations, and we will see if all the others are comprehended within the same law. It will only remain to determine a single value of the absolute dilatation between two known temperatures, which will be done by weighing; and with these data calculation will enable us to determine the true or apparent volume of the liquid at any temperature whatever.

Additions to the preceding Memoir.

(Read Aug. 13, 1813.)

The relations which I have established in the preceding memoir between the dilatations of different liquids and the degrees of the mercurial thermometer, are independent of every hypothesis. They enable us to determine by calculation the volume of each liquid at a temperature given by the thermometer; or reciprocally to calculate the temperature, the volume being given. This is all that observation requires.

The dilatation of mercury in glass is taken for a type, to which all the others are referred. We may equally refer the variable volume to any other dilatation. Its absolute values would remain the same; but the form of the function expressing it would change. This is what Mr. Dalton has done in his Philosophical Chemistry. This skilful philosopher having remarked that the dilatations of water increase nearly as the squares of the temperatures, reckoning from the maximum of condensation, conceives that the same thing ought to hold with all other liquids, whose composition remains constant during their change of volume; and that if this law of the square does not hold rigorously with water, it is because the expansion of the mercurial thermometer is not quite proportional to the heat. He conceived the idea of substituting for this thermometer an ideal thermometer, which possesses that property; such, for example, as we may conceive it in an air thermometer. He supposes that the dilatation of mercury, expressed in functions of the ideal thermometer, ought equally to follow the law of the squares, setting out from the point of congelation; and he thinks that in calculating in the same way the dilatations of all other liquids by the ideal thermometer, they will be all found subject to the same law.

This hypothesis gives immediately the form of the function which ought to express the correspondence between the mercurial and the ideal thermometer. Let us conceive these two thermometers regulated together at the extreme points of freezing and boiling water; let us suppose, likewise, that the interval between these two points is divided in each into 80 parts, as in the thermometer of Deluc. Then if we plunge the two instruments into the same liquid bath in which the first will mark T degrees, and the second *t*; the relation of T to *t*, according to the hypothesis, will of necessity be of this form:—

$$T = a' t + b' t^2$$

since it must set out from a maximum from which it varies as the square of the temperature. Let (t) be the true temperature of this maximum. We ought then to have

$$\frac{dT}{dt} = 0, \text{ or } a' + 2 b' (t) = 0$$

But as this must correspond with the freezing point of mercury, for which

$$T = -32^\circ \text{R}$$

we shall have likewise

$$-32 = a' (t) + b' (t)^2$$

The first of these equations gives

$$(t) = -\frac{a'}{2b'}$$

Substituting this value in the second, it becomes

$$32 = \frac{a'^2}{4b'} \text{ or } b' = \frac{a'^2}{128}$$

The two thermometers which coincided at 0° must coincide likewise at 80°. For this we must have at the same time

$$T = 80; \ t = 80$$

This gives the condition

$$a' + 80 b' = 1$$

This joined to the preceding determines *a* and *b*. We find in this manner very nearly

$$a' = \tfrac{184}{264}; \ b' = \tfrac{1}{264}$$

There is likewise another value of a', but it is not admissible, because it would make T diminish when t increases. By substituting these values in the general expression of T, we obtain

$$T = t + \frac{184}{364} t^2 + \frac{1}{164} t^3$$

This formula gives the correspondence of the mercurial thermometer with the ideal thermometer of Dalton. Accordingly the results deduced from it are the same as those which that skilful philosopher has given in his Tables of Temperature, p. 14. The first column of that table contains the number of degrees indicated by the ideal thermometer, which Mr. Dalton calls *true temperature*. The corresponding values of T form the third column of Mr. Dalton's table, supposing the degrees of Reaumur which we have employed to be converted into degrees of Fahrenheit. The next column gives the same degrees T affected by the dilatation of the glass. The point of the thermometric scale between 0° and 80° at which T differs most from t answers to $t = 40°$, which gives $T = 34°$. Hence at that point t differs 6° from T. Mr. Dalton finds for this difference 5·3°, probably on account of small fractions which we neglected in resolving the value of a' by approximation; and perhaps likewise because Mr. Dalton has established his calculations in a different manner, though from the same principles as we have done.

Now that this hypothesis is reduced to its simplest terms, a reflection naturally suggests itself, and it is this: that when considered in itself, it is extremely improbable, because it gives to mercury and all liquids a true maximum of condensation fixed at their point of congelation. The annunciation of the hypothesis leading to an expression of this form:

$$T = a' t + b' t^2$$

T has necessarily a maximum when

$$t = -\frac{a'}{2 b'}$$

For if we make

$$t = -\frac{a'}{2 b'} + t'$$

which supposes us to reckon the temperatures t' setting out from

$$t = -\frac{a'}{-2 b'}$$

we find

$$T = -\frac{a'^2}{4 b'} + t'^2$$

from which we see that the values of T are the smallest possible when t' is null, and continually increase from that term, at least if b' be positive. For mercury, for example, we shall have

$$\frac{a'^2}{4 b'} = -32$$

Consequently

$$T = -32 + t'^2$$

Thus, according to the hypothesis of Mr. Dalton, the common mercurial thermometer can never sink further than 32° Reaumur below 0°, which is the point of congelation of that liquid; but this is quite contrary to experience; for we know that all the liquids hitherto observed may, with certain precautions, be cooled below their point of congelation without becoming solid; and in that case they continue to follow the law of dilatation belonging to them. Thus water, for example, dilates equally on both sides of its maximum, whether heated or cooled, 10° R., reckoning from that point. And olive oil, which in the open air congeals at a very moderate cold, may be cooled down to — 14° Reaumur without ceasing to be liquid, as is obvious from the experiments of Deluc: and in this state it continues to contract according to the same law that it followed in other parts of the thermometric scale, because, as I have shown, that law excludes it from a maximum of condensation. The same holds with mercury, as is shown by the discussion of Mr. Cavendish respecting the experiments of Hutchins at Hudson's Bay; for it results from that discussion that mercury, like other liquids, may be cooled below its freezing point without becoming solid, and that this frequently happened in the experiments of Hutchins; and in these cases the mercury continued to contract gradually till the moment of solidification, when it suddenly underwent a much more considerable contraction. All these results are contrary to the law of dilatation supposed by Mr. Dalton. The same inconsistency exists with respect to all the liquids that contract progressively to the instant of their solidification.

If, notwithstanding these physical contradictions, we wish to examine Dalton's hypothesis relative to the expansion of water, which is the object that he had chiefly in view, we shall find that it corresponds much less accurately than the empyrical law deduced from the thermometrical observations. This is very natural, because these observations furnished us with a very delicate test, on which our formulas were moulded. It is obvious that a slight change in the thermometrical scale, such as that resulting from Dalton's hypothesis, between 0° and 80°, cannot produce a considerable effect upon a liquid which dilates as water does; and this will be the less sensible, as Mr. Dalton has in some measure compensated the excess of his scale of true temperatures, by the excess of temperature which he assigns to the point of maximum condensation; but the error may become greater when this scale is applied to other liquids, the dilatations of which are greater; for example, to alcohol; and this is what happens. Mr. Dalton himself has acknowledged that the law of dilatation deduced from his hypothesis cannot be reconciled to the thermometrical observations which Deluc made on this liquid, especially at high temperatures. Struck with this disagreement, he has been induced to call in question the accuracy of these observations; for, says he, as the dilatation of alcohol from 62° to 80° R. must have been conjectural, it is possible that Deluc may have exaggerated it; but the experiments of Deluc and of other philosophers have shown long ago that when a liquid is enclosed in a close vessel it is capable of supporting without boiling a much higher temperature than the point at which it boils in the open air; and the theory of Mr. Dalton on the formation of vapours gives a satisfactory reason for this fact. Accordingly alcohol thermometers have been long made capable of bearing the heat of boiling water.

We see from our formulas that the dilatation of alcohol in these extreme limits, instead of being irregular, continues conformable to itself, and follows the same law at the temperature of boiling water as at 10° Reaumur below zero; only, as this dilatation is not proportional to that of mercury, it is easy to conceive that its absolute value is not the same in different parts of the thermometrical scale for the same number of degrees of the thermometer. This is strikingly confirmed by an observation of Mr. Dalton himself on the absolute dilatation of alcohol in a glass vessel from − 17·78° R. to + 62·22°, which includes an interval of 80°. The dilatation in this interval ought not to be the same as from 0 to 80. In fact, by calculating from our formula, we find

From 0 to − 17·78 the true dilatation	−0·0209325
From 0 to + 62·22 the true dilatation	+0·0919566
Difference of total dilatation from − 17·78 to + 62·22	+0·1128891
Substracting the dilatation of the glass − 80 K....	0·0026272
We have the apparent dilatation..................	0·1102619
The value found by Dalton is	0·110

It is exactly the same with ours in the number of decimals that he has retained. This conformation of our formulas is so much the stronger, as no determination below zero entered into their construction, though I afterwards compared them with the experiments of Deluc made at − 10° R., in order to see if they would hold good at that point.

Mr. Dalton likewise gives us a confirmation in his work of the value which I have ascribed to the absolute dilatation of water between the temperatures of 0 and 80° R.; for he states it from experiment at 0·0466, precisely as I had deduced it from the thermometrical observations of Deluc, combined with a single determination of the weight of water by Blagden and Gilpin at the temperature of 30·22°.

Saline solutions and oils having very different dilatations from water, cannot agree with the hypothesis of Dalton. Accordingly, he excludes them from his hypothesis, which he confines to what he calls the simple liquids, such as mercury and water, which are the ones that agree with it best, though it is inconsistent with the physical properties of mercury. Yet we have seen that experiments made upon the most complex liquids, the oils, saline solutions, mixtures of water and alcohol, may be all represented by our formulas, and lose in them their apparent irregularity. Besides, as these formulas include no hypothesis, and are deduced solely from observations, it seems better to adhere to them, and to refer the dilatation of other liquids, as we have done, to that of mercury, as including the first three powers of the temperature which the thermometer indicates.

M. Laplace, whose views in physics are always so ingenious and general, has engaged me to examine whether it would not be possible to expunge the term depending on the cube of the temperature, by referring all the dilatations to an ideal thermometer, such that the mercurial thermometer itself should be expressed in a function of it in the same manner by a simple law of squares, reckoning for each liquid from a different point. But I have ascertained that this agreement is not general, at least with the coefficients which I have obtained; for their signs change for the different liquids as well as their values, so that it would be impossible to make the term depending on the cube of the temperature to disappear in all these liquids, by any single supposition respecting the dilatation of mercury in a function of an ideal thermometer. Perhaps experiments still more exact than those which I have used may enable us hereafter to discover a more simple law; but till that time come, the formulas which I have given will supply the occasions of observers.

ARTICLE IV.

On the Calculus of Variations. Translated from Traité de Calcul Integral, par Bossut. By Mr. George Harvey, of Plymouth.

(To Dr. Thomson.)

SIR,

THE following exposition of the theory of variations is translated from the Calcul Integral of Bossut. I should not request its insertion in your *Annals*, did I not conceive that its publication would be found eminently beneficial to the young mathematician.

I am, Sir, your humble servant,

Plymouth, April 26, 1817. GEORGE HARVEY.

Let there be any function whatever, composed of constant and variable quantities, which changes its value either by the *increase* or *decrease* of one or more of the elements which it contains. It will therefore undergo a *variation;* and the method of determining this *variation* is denominated the *calculus of variations.*

The variation of a function is designated by the Greek letter δ, in the same way as the differential is denoted by the Roman letter d; and the fundamental rules of the calculus of variations rest on the same principles as those of the differential calculus; but it is neces-

onfound a *variation* with a *differential.* An elementary
l exhibit the proper distinction between these two de-
f analysis.

II. Fig. 8.) Let $y^2 = a\,x$ represent the equation of a
M, referred to the rectangular co-ordinates A P (x),
ıd of which the parameter is a. By drawing $p\,m$ inde-
to P M, and M r parallel to the axis A V, the element
will represent the differential ($d\,x$) of the abscissa, and
$r\,m$ the differential ($d\,y$) of the ordinate. The relation
entials $d\,x$, $d\,y$, is found by differentiating the equation
hich gives $2\,y\,d\,y = a\,d\,x$; or $d\,y = \frac{a\,d\,x}{2\,y} = \frac{a\,d\,x}{2\sqrt{a\,x}}$.

xt place, let it be conceived that the equation $y^2 = a\,x$
e indefinitely small increase $\delta\,a$ of the parameter a,
: of its elements; and let a second parabola A N be
which has $a + \delta\,a$ for its parameter. Then by sup-
, that the abscissa A P remains the same for both para-
manifest that the ordinate P N, of the parabola A N,
esented by the primitive ordinate P M increased by the
N, and will therefore represent the *variation* which the
M receives, in consequence of the *variation* of the

.

enting, therefore, the *variation* of y by $\delta\,y$, as that of
it is necessary, in order to form the equation of the
N, to substitute $y + \delta\,y$ for y, and $a + \delta\,a$ for a in the
$= a\,x$, which gives $(y + \delta\,y)^2 = x\,(a + \delta\,a)$. Sub-
primitive equation $y^2 = a\,x$ from the preceding equa-
eglecting the *variations* of the *second order*, as in the
fferentials, we shall have $2\,y\,\delta\,y = x\,\delta\,a$, or $\delta\,y = \frac{x\,\delta\,a}{2\,y}$

an equation which exhibits the relation of the *varia-*

y.

abscissa A P be increased by the element P p ($\delta\,x$), the
ıg ordinate of the parabola A N will be $q\,n$, and the
will represent the *variation* of the primitive ordinate
ind the equation which expresses the relation of the
a, $\delta\,x$, $\delta\,y$, in the equation $y^2 = a\,x$, let $y + \delta\,y$ be
or y, $x + \delta\,x$ for x, and $a + \delta\,a$ for a, and we shall
$y)^2 = (a + \delta\,a)\,(x + \delta\,x)$; from which subtracting
equation $y^2 = a\,x$, and neglecting the *variations* of
der, we then have $2\,y\,\delta\,y = x\,\delta\,a + a\,\delta\,x$; and there-
$\frac{\delta\,a + a\,\delta\,x}{2\,y} = \frac{x\,\delta\,a + a\,\delta\,x}{2\sqrt{a\,x}}$, an expression of the value
variation $s\,n$ of the ordinate.

ıUM I.—In this example, and indeed in all others of a
e, the parameter a, and its variation $\delta\,a$, are constant

for the whole extent of the two parabolas, whilst the co-ordinates A P (x) and P M (y) continually change. *The changes relative to the same parabola are related to* DIFFERENTIALS; *while those which result from the passage of one parabola to another are related to* VARIATIONS.*

Each of the *variations* δa, δx, δy, may be taken arbitrarily: thus, for example, we may suppose $\delta a = d x$; but when this condition is once made, the values of the other *variations* are subordinate to it; and it is not allowed afterwards to make $\delta y = d y$, or $\delta y = \delta a$.

4. SCHOLIUM II.—There is no particular difficulty in determining the *variations* of all orders of *algebraic*, *exponential*, and *circular* functions. To obtain the *variation* of a function, it is merely necessary to write δ in place of d, the symbol of differentiation; and in this respect the calculus of *variations* corresponds with the differential calculus. But the principles of the differential calculus are not sufficient, when it is required to determine the *variations* of functions, which contain the signs of integration, when those integrations are not capable of being effected. For example, let $\int V\, d x$ be an expression in which V is a given function of x, y, z, &c. and constant quantities; we differentiate by omitting the symbol $\int$, which gives $V\, d x$ for the differential; but the expression of the *variation* $\delta \int V\, d x$ is very different. *Now the principal object of the calculus of variations is to determine the variations of integral formulæ of this species;* and we proceed, therefore, to establish the principles which ought to serve as the basis of this department of analysis.

FIRST PRINCIPLE.

The *variation* of a *differential* is equal to the *differential* of the *variation*, and reciprocally; that is to say, $\delta (d \Pi) = d (\delta \Pi)$.

For let it be supposed that the variable function Π represents the ordinate of a curve; this ordinate changes by *differentials* in the *same* curve; but by *variations in passing from the proposed curve to another indefinitely near to it.* Let Π' be the consecutive value to Π, for the first curve, and consequently $\Pi' = \Pi + d \Pi$, or $d \Pi = \Pi' - \Pi$. Taking the *variations* of this last equation, it will become $\delta (d \Pi) = \delta \Pi' - \delta \Pi$. But since Π and Π' are consecutive quantities in the series of Π, we may regard $\delta \Pi$, $\delta \Pi'$, as consecutive quantities in the series of $\delta \Pi$; so that $\delta \Pi' = \delta \Pi + d (\delta \Pi)$; or $d (\delta \Pi) = \delta \Pi' - \delta \Pi$. Thus by equating these two values of $\delta \Pi' - \delta \Pi$, there will arise $\delta (d \Pi) = d (\delta \Pi)$.

COROLLARY.—If, therefore, a function containing any number of d's and δ's, which affect the *same* variable, we may make these characteristics change place at pleasure; for we have found $\delta (d \Pi) = d (\delta \Pi)$; consequently $\delta (d^2 \Pi) = d (\delta (d \Pi)) = d^2 (\delta \Pi)$; and $\delta (d^3 \Pi) = d (\delta d^2 \Pi) = d^2 (\delta d \Pi) = d^3 (\delta \Pi)$; &c. &c.

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* This remark is important to the student.

SECOND PRINCIPLE.

The *variation* of an *integral* formula is equal to the *integral* of the *variation* of its *differential*; that is to say, $\delta(\int\xi) = \int(\delta\xi)$.

Let $\int\xi = z$, and consequently $\xi = dz$; hence by taking the variations $\delta\xi = \delta(dz) = d(\delta z)$. Integrating this last equation, we shall have $\int(\delta\xi) = \delta z = \delta(\int\xi)$.

COROLLARY.—Therefore in repeated integrations we may change the signs $\int$ and δ at pleasure; for we have found $\delta(\int\xi) = \int(\delta\xi)$; therefore $\delta(\int\int\xi) = \int(\delta\int\xi) = \int\int(\delta\xi)$.

Similarly $\delta(\int\int\int\xi) = \int(\delta\int\int\xi) = \int\int(\delta\int\xi) = \int\int\int(\delta\xi)$, &c.

PROBLEM.

To determine the *variation* of any indefinite integral formula $\int\Pi\,dx$:—

Whatever the function Π may be, we have always by the second principle $\delta(\int\Pi\,dx) = \int\delta(\Pi\,dx)$; but $\delta(\Pi\,dx) = dx\,\delta\Pi + \Pi\,\delta\,dx$; and the first principle gives $\delta(dx) = d(\delta\Pi)$; therefore $\delta\int\Pi\,dx = \int dx\,\delta\Pi + \int\Pi\,d\,\delta x$. But by the method of integrating by parts,* the last term $\int\Pi\,d\,\delta x = \Pi\,\delta x - \int d\Pi\,\delta x$; and therefore by substitution $\delta\int\Pi\,dx = \Pi\,\delta x + \int dx\,\delta\Pi - \int d\Pi\,\delta x$; or $\delta\int\Pi\,dx = \Pi\,\delta x + \int(dx\,\delta\Pi - d\Pi\,\delta x)$.

ARTICLE V.

Determination of the Thickness of Wall necessary to support a given Arch. By Mr. James Adams.

(To Dr. Thomson.)

SIR, *Stonehouse, April* 20, 1817.

IF in your opinion the following question and solutions merit a place in the *Annals of Philosophy*, your inserting them therein will much oblige your humble servant,

JAMES ADAMS.

The Question.

A B H D (Pl. LXVII. Fig. 9) represent a vertical section of half a brick arch and work over its top, and D E F G a vertical section of the perpendicular side wall of brick also, the slope E H being parallel to the chord A B; the angle B A O = 30°, semi-span

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* Since by the theory of differentials $d\,.\,xy = xyd + ydx$, integrating and transposing, it becomes $\int x\,dy = xy - \int y\,dx$, which is the general formula for integrating by parts.

A O = 7 feet, height A G = 10 feet, line R S = 12·8 feet, and B H = A D = 2·75 feet; from hence it is required to find the thickness of the wall F G necessary to support the said arch and work on its top.

The data in the question are collected from a building that fell just as completed.

First Solution.

Let R (Fig. 10)* represent the centre of the space A B H D and I the centre of the arc A B; draw K L perpendicular, and I P parallel to A O; join I K, and draw K N perpendicular thereto; draw L M, F N, parallel to I K, and K Q parallel to A O. Then if K L represent the area A B H D, K M will represent the force acting at N, at right angles to F N, and by the similar triangles I K P, K L M, C K Q, and C F N, we have, after making

K L .... = 3·8 Feet
O I = O B .... = 4·0414 Ditto
K P .... = 7·8414 Ditto = a
I P = O L .... = 3·4 Ditto = b
I K^2 .... = $a^2 + b^2$ Ditto = c^2
K T = L A .... = 3·6 Ditto = d
Area A B H D ...... = 13·333 Ditto = m
F Q = G A + A T .. = 13·7 Ditto = f
R S .... = 12·8 Ditto = g
Q T = F G .... = x Ditto
K Q = K T + T Q .. = $d + x$ Ditto

$$IK : IP :: LK : KM = \frac{LK \times IP}{IK} = \frac{bm}{c}$$

$$KP : PI :: KQ : QC = \frac{KQ \times PI}{KP} = \frac{(d+x)\,b}{a}$$

$$FQ - QC = FC = f - \frac{(d+x)\,b}{a} = \frac{af - (d+x)\,b}{a}$$

$$IK : KP :: FC : FN = \frac{FC \times KP}{IK} = \frac{af - (d+x)\,b}{a} \times \frac{a}{c} = \frac{af - (d+x)\,b}{c}.$$

Then, per mechanics,

$$RS \times FG \times \tfrac{1}{2} FG = FN \times KM; \text{ that is,}$$

$$\frac{gx^2}{2} = \frac{af - (d+x)\,b}{c} \times \frac{bm}{c} \therefore x^2 + \frac{2mb^2}{gc^2}x = (af - bd)\frac{2bm}{gc^2}.$$

In numbers $x^2 + \cdot 3297\, x = 9{\cdot}23074 \therefore x = 2{\cdot}878$ feet, the required thickness according to this solution.

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* The centre of gravity K was found as follows: I constructed the mixed lined space A B H D very accurately to a large scale, and subdivided it by lines drawn parallel to B H, equidistant from each other, and *one foot apart*; then found geometrically the centre of gravity of each subdivision, by considering them as so many trapeziums. From the same scale, and from the consideration of moments, I found O L = 3·4 feet, and L K = 3·8 feet.

Corollary.—When F N = R S, then $FG = \sqrt{2KM} = \sqrt{\frac{2bm}{c}} = \sqrt{\frac{6\cdot8 \times 13\cdot333}{8\cdot5464}} = 3\cdot257$ feet.

Second Solution.

If F L, as before, represent the area A B H D, L A will denote the force acting at A, at right angles to A G, and is thus determined. $KL : LA :: m : \frac{m \times LA}{KL}$ = horizontal force acting at A.

Then, per mechanics,

$$RS \times FG \times \tfrac{1}{2}FG = AG \times \frac{m \times LA}{KL} \therefore FG = \sqrt{\frac{2m \times LA \times AG}{KL \times RS}}$$

In numbers, $FG = \sqrt{\frac{2 \times 13\cdot333 \times 3\cdot6 \times 10}{12\cdot8 \times 3\cdot8}} = 4\cdot4426$ feet, the required thickness according to this solution.

Corollary.—When A L = L K, then $FG = \sqrt{\frac{2m \times AG}{RS}} = \sqrt{\frac{20 \times 13\cdot333}{12\cdot8}} = 4\cdot5643$ feet.

The thickness of the wall alluded to in the question was *three feet and two inches.*

Hence it appears that the thickness 2·878 feet, as determined by the first solution, would not have been sufficient to resist the force of the arch; but if the walls had been made 4·4426 feet thick, as determined by the second solution, I am of opinion the failure would not have happened. Notwithstanding the *second solution* gives a thickness for the walls the most likely to ensure stability in the present instance, it must not be understood that this method is preferable to the former in all cases; for when the extrados is *horizontal,* Dr. Hutton, on referring to the principles contained in the *first* solution, makes the following remark :—

"It may be presumed that this theorem brings out the thickness of the piers very near the truth, and very near what would be allowed in practice by the best practical engineers, as may be gathered from a comparison of the two cases of Westminster and Blackfriars Bridges; in the former of which the centre arch is a semicircle of 76 feet span and 17 feet thickness of piers; and in the latter it is a semiellipse of 100 feet span, and 40 feet in height, and 19 feet thickness of piers." (Dr. Hutton's Tracts, vol. i. p. 81.)

$$\tfrac{1}{5} \text{ of } \left\{\begin{matrix} 14 \\ 76 \\ 100 \end{matrix}\right\} \text{ is } \left\{\begin{matrix} 2\cdot8 \\ 15\cdot2 \\ 20\cdot0 \end{matrix}\right\} \begin{matrix}\text{not widely} \\ \text{differing} \\ \text{from}\end{matrix} \left\{\begin{matrix} 2\cdot878 \\ 17\cdot000 \\ 19\cdot000 \end{matrix}\right\} \text{as herein stated.}$$

Article VI.

On a Preparation of Cinchona. By C. Johnson, one of the Surgeons to the Lancaster Dispensary.

(To Dr. Thomson.)

SIR,

One would suppose that Count Rumford's elaborate essay on the subject of coffee would at least have brought his elegant apparatus for preparing that beverage into the shops of those who manufacture such articles for sale; but none could be procured in London by one of my friends, who made the proper inquiries with great diligence.

I have for some time adopted this excellent method of making coffee, and extended its application to Peruvian bark. Several medical friends to whom I have shown this infusion (or rather perhaps perfusion) of cinchona have readily adopted it, and requested me to render more public what they think an improvement on pharmacy.

The machine I use is similar to one made several years ago by Edmund Loyd and Co. 178, Strand; and does not differ essentially from any of those described in Count Rumford's 18th essay, and in the Repertory of Arts for April and May, 1813.

Peruvian bark pounded and sifted through a wire sieve is placed in the strainer of this apparatus, and boiling water is poured upon it in successive portions. When about a quart of water has been thus passed through an ounce of cinchona, it forms a beautiful, clear solution of all that water can extract, and strongly exhibiting the sensible properties of cinchona.

Dr. Duncan, jun. has long ago suggested that the precipitate which decoction of bark deposits on cooling may prove an useful and compendious medicine. This suggestion seems well founded, and deserves the trial. Those inclined to examine this matter may obtain a very abundant supply on cooling the strong infusion which first runs off.

A little reflection on the chemical properties of cinchona, and a comparison of the infusion and decoction as usually prepared with the infusion just described will convince your readers that the last preparation must afford an useful and efficient medicine. The saving effected in this way will be a small matter of consideration with medical practitioners as far as their interest only is concerned; but in dispensaries, infirmaries, and the public service, no item of expense is unimportant: and since on some occasions a scanty supply of cinchona has been felt as a national calamity, it becomes a duty to prevent a wasteful consumption of this valuable remedy. On this account I mention that in the Lancaster Public Dispensary this method is found to afford a better preparation than was formerly obtained from twice the quantity of cinchona.

The stratum of bark should be of such a thickness that the water may neither pass through too slowly nor too rapidly. I have found a strainer of two inches, three inches, or four inches diameter, most suitable for ½ oz., 1 oz., or 2 oz. of cinchona.

I remain, Sir, your obedient servant,

Lancaster, April 15, 1817. C. JOHNSON.

ARTICLE VII.

Abstract of a Memoir entitled Examination of some Minerals found in the Neighbourhood of Fahlun, and of their Situation. By J. G. Gahn, J. Berzelius, C. Wallman, and H. P. Eggertz. Inserted in the fifth Volume of the Afhandlingar i Fysik, Kemi och Mineralogi.*

THE neighbourhood of Fahlun being remarkable for the great variety of uncommon minerals which have been found in it, Messrs. Gahn, Berzelius, Wallman, and Eggertz, undertook to examine them, both in a mineralogical and geognostic point of view; and in the excursions which they made last summer for that purpose their attention was fixed chiefly on the excavations at Finbo.

While analyzing the deuto-fluate of cerium, and the double fluate of cerium and yttria, Berzelius found in them a new earth, which he had extracted the preceding year from the gadolinite of Korarvet, but in too small quantity to be able at that time to determine its properties with the requisite precision. I shall extract from the memoir in question every thing that concerns the new earth.

Minerals in which the new Earth is found.

The neutral *deuto-fluate of cerium* of Finbo is of a deeper red than that of Broddbo. It is sometimes found crystallized in six-sided prisms, the length of which exceeds the breadth, sometimes in plates more or less thin, and sometimes in irregular amorphous masses. It is imbedded in a rock composed of albite, quartz, and mica, and is accompanied by emeralds and yttro-tantalite; but it occurs so sparingly that all the specimens which we found were hardly sufficient for a single analysis. I satisfied myself, therefore, with ascertaining by small experiments that it is a neutral fluate of cerium; and by means of the blow-pipe I have satisfied myself that its deeper colour is owing to the presence of a greater proportion of manganese.

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* This volume has not yet been published. For the present abstract I am indebted to the Chevalier d'Obsson, who kindly translated it from the Swedish original, of which he was possessed of a copy.—T.

The rarest variety is that which is amorphous, and presents no marks of crystallization. Some of the experiments made upon it deserve to be stated here, though they cannot be considered as exhibiting an exact analysis.

A. Forty-eight parts of it being reduced to an impalpable powder, and calcined in a red heat, were submitted to the action of concentrated sulphuric acid, which occasioned the separation of the fluoric acid gas, and converted the mass into a semiliquid substance of a fine deep brown colour. After two hours' digestion it was brought in contact with a little water, which occasioned a slight muddiness. The yellow liquid was decanted off, and mixed with hot water, which occasioned still greater opacity. The precipitate, being collected on the same filter as the undissolved portion, and being washed and heated to redness, weighed 9·6 parts.

B. The liquid was mixed with sulphate of potash till the whole of the cerium separated. When properly washed and dried, the oxide of cerium obtained weighed 26·3 parts.

C. The solution was then treated with ammonia. The resulting precipitate weighed, after calcination, 1·525 parts; and I found by an examination which I conceive it to be unnecessary to state separately, a mixture of yttria, alumina, oxide of manganese, and silica.

D. The 9·6 parts that had not been dissolved by sulphuric acid were digested at the temperature of boiling water in muriatic acid, which dissolved them with the exception of 2·5 parts, which were silica mixed with a trace of proto-fluate of cerium.

E. The muriatic solution was mixed with caustic ammonia. The precipitate, being thrown upon a filter, was well washed, and dissolved while still moist in nitric acid; and this solution was left to evaporate spontaneously in a warm place. It produced a gummy mass, deliquescing in the air, which, being dissolved in a greater quantity of water, and boiled, let fall a white gelatinous precipitate, which was collected on the filter. It weighed three parts. Caustic ammonia, being mixed with the remaining solution, precipitated oxide of cerium, which still contained a portion of the earth precipitated by boiling. I shall describe below the experiments made on this earth.

The analysis, then, had assigned oxide of cerium as the principal substance, and had given the total quantity of 37·4 of solid matter. The loss, amounting to 10·6, greatly exceeds the quantity of fluoric acid which was requisite to saturate the different bases. This excess of loss is no doubt owing to the fluoric acid having carried off with it a portion of silica, which in all probability was only mechanically mixed, as it is in the minerals which I shall mention immediately.

Double Fluate of Cerium and Yttria.—There is an earthy mineral found at Finbo which is much more common than the neutral fluates and subfluates of cerium; but its size seldom exceeds that of a pea. Its most usual colour is pale red, similar to that of a mixture of carmine and white lead; but it is sometimes white, or deep red, or nearly yellow. It is so soft that it may be easily scratched by the nail, and it may be easily detached from its matrix by the fingers. It then leaves a rough irregular cavity.

This mineral occurs likewise in irregular amorphous masses of a reddish-brown colour, sometimes separate, sometimes surrounding gadolinite, or mixed with it so as to appear a part of it. It never shows any tendency to assume a regular figure, or a crystalline texture.

I have made several analyses of this mineral, which have all given different results; which shows that the relative quantities of its constituent parts are very variable.

While analyzing a specimen of this mineral, which did not differ in its external appearance from other specimens, I found a new quantity of the same earth which had been extracted from the amorphous neutral deuto-fluate of cerium. I shall state briefly this experiment.

Twenty-two parts of the pulverized mineral were treated with sulphuric acid, which decomposed it, with the exception of 3·5 parts. The solution was mixed with sulphate of potash, to separate the oxide of cerium. It weighed two parts. I then added caustic ammonia. The precipitate which fell, being heated to redness, weighed 15·5 parts. I poured muriatic acid on it, which readily dissolved a portion of it. The residue was only dissolved by means of a long digestion. The liquid was evaporated to dryness by means of a gentle heat, in order to drive off the excess of acid. I then poured water over it, which dissolved the muriate of yttria. The residue was dissolved in muriatic acid, and the liquid was saturated by caustic ammonia as accurately as possible. I then added water, and caused it to boil. A white gelatinous precipitate fell, which was collected on the filter. The liquor that passed through the filter was again saturated by caustic ammonia, and heated to ebullition, which occasioned the precipitation of a new portion of the same earth. When washed and slightly dried, it weighed seven parts. In the 3·5 parts of yttria separated from the 15·5 parts I found, by means of caustic potash, a small quantity of alumina; the weight of which, however, I did not exactly determine.

Particular Examination of the new Earth.

While I was examining the composition of gadolinite during the summer of 1815, I obtained in one of my analyses a peculiar substance, amounting to 30 per cent. of the weight of the mineral, and which possessed properties different from the other earths. It was absolutely similar to the substance just found at Finbo. It was separated from gadolinite in the following manner:—The mineral having been dissolved in nitro-muriatic acid, the filtered solution was saturated by caustic ammonia, and precipitated by succinate of ammonia, having a slight excess of acid. The liquor being filtered, I mixed it with sulphate of potash, which occasioned a precipitate. Before separating the yttria, I wished to prevent the oxide of manganese from being deposited along with it. For this purpose I filtered into the liquid a boiling solution of muriate of ammonia, in order to form a double salt composed of muriate of ammonia and proto-muriate of manganese, which would prevent this last oxide from being precipitated by the ammonia. The consequence was a bulky white deposite. I continued to pour in the salt till all precipitation was at an end. The precipitate was thrown upon a filter, washed, and dried. Perceiving that it was a substance different from any which I expected to find in gadolinite, I wished to prepare a greater quantity of it. But though I endeavoured with the greatest care to ascertain all the external differences which the specimens of gadolinite from Korarvet exhibited, and examined each of them separately, I could not obtain the smallest trace of the substance, though I had fallen upon pretty correct methods of separating it from yttria and oxide of cerium, even when it existed only in small proportions. I therefore deferred to a future opportunity further researches on this substance, without even mentioning it in the analysis of that variety of gadolinite, because I still considered its existence as problematic. Having found it again at Finbo, I endeavoured to determine its properties more exactly; but as it happens here also that the same mineral does not always contain it, or that minerals which contain it are absolutely similar to those which do not contain it, I could not be sure at present of obtaining a new portion of it, without destroying a great part of the specimens of a mineral which is very rare. I have thought it right, therefore, in the present uncertainty, to describe it such as I have found it, that if it be discovered hereafter in greater abundance, as it is probable it will, my statements may facilitate the means of separating and examining it. I may state here, by way of apology for the imperfection of this notice, that I have not had altogether half a gramme of this earth to make my experiments with.

To obtain it from those minerals that contain protoxide of cerium and yttria, we must first separate the oxide of iron by succinate of ammonia. The new earth, indeed, may, when alone, be precipitated by the succinates; but in the analytical experiments in which I have obtained it, it precipitated in so small a quantity along with iron, that I could not separate it from that oxide. The deutoxide of cerium is then precipitated by the sulphate of potash; after which the yttria and the new earth are precipitated together by caustic ammonia. Dissolve them in muriatic acid. Evaporate the solution to dryness, and pour boiling water on the residue, which will dissolve the greatest part of the yttria; but the undissolved residue still contains a portion of it. Dissolve it in muriatic or nitric acid, and evaporate it till it becomes as exactly neutral as possible. Then pour water upon it, and boil it for an instant. The new earth is precipitated, and the liquid contains disengaged acid. By saturating this liquid, and boiling it a second time, we obtain a new precipitate of the new earth.

This earth, when separated by the filter, has the appearance of a gelatinous, semitransparent mass. When washed and dried, it becomes white, absorbs carbonic acid, and dissolves with effervescence in acids. Though calcined, it retains its white colour; and when the heat to which it has been exposed was only moderate, it dissolves readily in muriatic acid; but if the heat has been violent, it will not dissolve till it be digested in strong muriatic acid. This solution has a yellowish colour; but it becomes colourless when diluted with water, as is the case with glucina, yttria, and alumina. If it be mixed with yttria, it dissolves more readily after having been exposed to heat. The neutral solutions of this earth have a purely astringent taste, which is neither sweet, nor saline, nor bitter, nor metallic. In this property it differs from all other species of earths except zirconia.

When dissolved in *sulphuric acid* with a slight excess of acid, and subjected to evaporation, it yields transparent crystals, which are not altered by exposure to the air, and which have a strong styptic taste.

The mother water remaining after the formation of these crystals retains but very little of the earth. When the crystals are exposed to the action of water, they are entirely decomposed. The solution becomes muddy, a subsulphate precipitates, and a supersulphate remains in solution. When this solution is boiled, it lets fall no precipitate. If the crystals of the sulphate of the new earth are exposed to the action of water in a state of rest, the subsulphate which remains undissolved retains the form of the crystals; but upon the least movement they fall to powder. The acid solution, when mixed with sulphate of potash till saturation, does not let fall any precipitate; neither does any precipitate appear when sulphate of potash is dropped into the muriate of this earth. If the liquid be raised to the boiling temperature, a portion of the earth precipitates in the state of subsulphate, and a portion remains in the liquid, which may be precipitated by means of caustic ammonia.

This earth dissolves very easily in nitric acid; but, after being heated to redness, it does not dissolve in it except by long boiling. The solution does not crystallize, but forms a mucilaginous mass, which becomes more liquid by exposure to the air, and which, when evaporated by a moderate heat, leaves a white, opake mass, similar to enamel, in a great measure insoluble in water. When the neutral solution of this nitrate in water is boiled, a great portion of the earth is precipitated. If the solution contain an excess of acid, it allows a portion of the earth to precipitate when it is diluted with water and boiled. A slight calcination of the nitrate leaves the earth with its white colour, so that we discover no evidence of a higher degree of oxidizement.

It dissolves in muriatic acid, in the same manner as in nitric acid.

The solution does not crystallize. When evaporated by a moderate heat, it is converted into a syrupy mass, which does not deliquesce in the air, but dries, becomes white like enamel, and afterwards dissolves only in very small quantity in water, leaving a subsalt undissolved; so that by spontaneous evaporation it lets the portion of muriatic acid escape to which it owed its solubility. A solution of this muriate, not too acid, and diluted with water, when raised to the boiling temperature, lets fall the greatest portion of the earth in the form of a gelatinous mass, light, and semitransparent. A solution of this earth in muriatic or nitric acid, when evaporated by a strong heat, leaves on the edges of the vessel a white, opake film, having the appearance of enamel. It appears very distinctly when the liquid is made to pass over the inside of the glass. This is a very characteristic mark of this earth; and I am not aware that it belongs to any other substance, except to the solution of phosphate of iron in nitric acid, which however does not present the phenomenon in so eminent a degree. I have been able from this layer of enamel to determine very well beforehand whether the mineral which I was analyzing contained this earth or not. This mark, however, is less evident when the earth is mixed with a considerable quantity of yttria and protoxide of cerium.

This earth combines with avidity with carbonic acid. The precipitates produced by caustic ammonia, or by boiling the neutral solutions of the earth in acids, absorb carbonic acid from the air in drying. The alkaline carbonates precipitate the earth combined with the whole of their carbonic acid.

The *oxalate of ammonia* throws down a white, voluminous precipitate, insoluble in water as well as in caustic alkalies.

The *tartrate of ammonia* produces a white precipitate, which redissolves at first, and does not become permanent till a sufficient quantity of the salt has been added. This precipitate is redissolved by caustic ammonia. Boiling drives off the ammonia; but the earth is not deposited till the liquid has been concentrated to a certain degree by evaporation. It then precipitates under the form of a gelatinous mass, almost transparent.

The *citrate of ammonia* does not occasion any precipitate, not even when caustic ammonia is added to it; but if the liquid be boiled, the earth precipitates in proportion as the ammonia evaporates. This precipitate is analogous to those that are produced by boiling in the other neutral solutions of this earth.

The *benzoate of ammonia* produces a white, bulky precipitate.

The *succinate of ammonia* occasions a precipitate, which is immediately redissolved. If a sufficient quantity be added to prevent the precipitate from redissolving, and if we attempt to redissolve it by pouring in water, it is decomposed, and remains in a great measure undissolved, under the form of a salt with excess of base, while the liquid contains the greatest part of the acid united to a small portion of the earth.

The *ferruginous prussiate of potash* poured into a solution of this earth, throws down a white precipitate, which is completely redissolved by muriatic acid.

Caustic potash and ammonia have no action on this earth newly precipitated, not even at a boiling temperature.

The solution of carbonate of potash or carbonate of ammonia dissolves a small quantity of it, which precipitates again when the liquid is supersaturated with an acid, and then neutralized by caustic ammonia; but this earth is much less soluble in the alkaline carbonates than any of the earths formerly known that dissolve in them.

A portion of this earth weighing 12 parts was exposed in a charcoal crucible to the heat employed to reduce tantalum, and the fire was kept up for an hour. When withdrawn, it did not appear to have undergone any other alteration than to have contracted in its dimensions, and to have acquired a slight transparency, having probably been near the fusing point. It exhibited no appearance of reduction, and was dissolved by boiling in muriatic acid. As it is at present generally known that the salifiable bases are metallic oxides, it may appear indifferent whether we say *earth* or *metallic oxide*. But these substances being divided into alkalies, earths, and metallic oxides, the proper method seems to be to attach every new link of the chain of oxides to those with which it has the greatest analogy. And since the earths are distinguished chiefly by being colourless, and by being irreducible when heated with charcoal without the assistance of a foreign metal, I consider the substance which has been just described as belonging particularly to the class of earths.

Although the experiments of which I have just given an account cannot certainly be considered as more than preparatory to a more complete examination of this earth, when a greater quantity of it is found, I have thought that it would be convenient to give it a name, that it might be pointed out more easily. A part of these experiments having been made in the laboratory of Mr. Gahn, at Fahlun, we were accustomed to speak of it to each other under the appellation *thorina*, from *Thor*, an ancient Scandinavian deity. It may, therefore, not be unsuitable to distinguish it provisionally by this denomination.

Thorina does not fuse before the blow-pipe. With borax it melts into a transparent glass, which, when exposed to the exterior flame, becomes opake and milky. With phosphate of soda it fuses into a transparent pearl. It is infusible with soda. When soaked with a solution of cobalt, it becomes greyish-brown.

Thorina differs from the other earths by the following properties:

From *alumina*, by its insolubility in hydrate of potash; from *glucina*, by the same property; from *yttria*, by its purely astringent taste without any sweetness, and by the property which its solutions possess of being precipitated by boiling when they do not contain too great an excess of acid. It differs from zirconia by the following properties: 1. After being heated to redness, it is still capable of being dissolved in acids. 2. Sulphate of potash does not precipitate it from its solutions, while it precipitates zirconia from solutions containing even a considerable excess of acid. 3. It is precipitated by oxalate of ammonia, which is not the case with zirconia. 4. Sulphate of thorina crystallizes readily, while sulphate of zirconia, supposing it free from alkali, forms, when dried, a gelatinous, transparent mass, without any trace of crystallization.

As thorina has a greater analogy with zirconia than with any other body, and as the two earths occur together at Finbo, it may be useful perhaps to exhibit here a parallel between their properties:—

Thorina.	Zirconia.
Taste of the neutral solutions purely astringent.	The same.
Crystallizes easily with sulphuric acid. The crystals are decomposed by water.	Does not crystallize, but becomes mucilaginous; and when long exposed to a moderate heat becomes white, opake, saline. Deliquesces in the air; but becomes muddy when water is poured into it, unless the solution be very acid. The dried salt can bear a moderate heat, without being more than partially decomposed.
The muriatic solution gives a precipitate when boiled. This precipitate is bulky, semitransparent, and gelatinous. Muriate of thorina does not crystallize.	The muriatic solution is precipitated by boiling. The precipitate is a heavy, white, opake powder. Muriate of zirconia crystallizes by evaporation.
The nitric solution, when boiled, lets fall a gelatinous earth.	The same.
Alkaline *succinates*, *benzoates*, and *tartrates*, occasion a precipitate in the solution of thorina. The precipitate by an alkaline tartrate is dissolved by hydrate of potash.	The same.
The *citrates* occasion no precipitate; but a precipitate appears when the liquid is boiled.	The *citrates* occasion no precipitate, nor does one appear when the liquid is boiled.
Oxalate of ammonia precipitates thorina from its solution in sulphuric acid.	*Oxalate* of ammonia throws down no precipitate from sulphate of zirconia.
Sulphate or muriate of thorina dissolved in water, and mixed to saturation with sul-	A salt of zirconia dissolved in water, and mixed to saturation with sulphate of potash, is en-

Thorina.	**Zirconia.**
phate of potash, lets fall no precipitate.	tirely precipitated. If this is done in the cold, the precipitate is entirely soluble in water.
Insoluble in hydrate of potash.	The same.
Soluble in alkaline carbonates.	The same, but in much greater quantity.
Becomes by calcination difficult of solution.	When heated to redness, becomes insoluble.

These two earths exhibit the same properties before the blow-pipe.*

I have reason to presume that the thorina found in the mineral from Korarvet, which I analyzed, was in the state of a *silicate*, similar to gadolinite; but that the portion found at Finbo was in a state of combination with fluoric acid.

Article VIII.

Magnetical and Meteorological Observations.
By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 42″ North. Longitude west in time 1′ 20·7″.

Magnetical Observations, 1817. — *Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
April 18	8h 45′	24° 30′ 51″	1h 45′	24° 44′ 38″	6h 45′	24° 35′ 23″
19	8 45	24 30 33	1 45	24 44 54	6 45	24 35 47
20	8 45	24 31 05	1 55	24 42 00	6 45	24 35 13
21	8 45	24 30 08	1 45	24 44 58	6 45	24 35 36
22	8 45	24 33 53	1 45	24 42 16	6 40	24 36 18
23	8 40	24 32 32	1 45	24 43 48	6 45	24 35 50
24	8 45	24 33 21	1 45	24 46 58	6 45	24 37 06
25	8 40	24 32 57	1 45	24 45 31	6 45	24 35 37
26	8 45	24 32 46	1 45	24 43 06	6 45	24 33 26
27	8 45	24 34 14	1 45	24 45 40	6 45	24 35 33
28	8 45	24 29 27	1 45	24 40 08	6 45	24 33 37
29	8 35	24 30 39	1 55	24 40 37	6 45	24 34 01
30	8 45	24 30 21	1 55	24 42 00	— —	— — —
Mean for Month.	8 44	24 31 52	1 46	24 44 43	6 40	24 35 58

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* I have read somewhere that zirconia gives a blue colour with cobalt, and hoped that this would furnish a ready method of distinguishing the two earths: but the blue colour does not appear except when the zirconia contains an alkali. When pure zirconia is obtained by expelling the acid from sulphate of zirconia by heat, it does not enter into fusion nor become blue with cobalt, but greyish-brown.

April 16. The needle at intervals was attracted to the eastward or repelled to the westward, and remaining there stationary for a minute or two, returned to its former place: the wind was unsteady from the NW, accompanied with showers.—26. The needle was similarly affected, and the wind was in the same point: the subsequent day apparently thunder showers were seen in different parts; but no thunder was heard, neither did any rain fall at Bushey Heath: the wind was unsteady from the NE, and during the night of the 26th it blew hard.—April 30, in the evening, the variation was 24° 44′ 15″, it decreased to 24° 16′ 45″, and again increased to 24° 47′ 50°: for this unusual variation there appears no cause: several black dense clouds were visible, and the weather squally, with showers; and during the day there were several violent hailstorms.

Meteorological Table.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.
		Inches.				Feet.	
Ap. 18	Morn........	29·935	40°	50°	NNE		Fine
	Noon........	29·950	50	44	E	5·332	Fine
	Even	29·980	47	48	Calm		Fine
19	Morn........	29·992	42	52	NW by N		Clear
	Noon........	29·995	51	44	NE to NW	7·598	Fine
	Even	29·975	47	48	NW by N		Clear
20	Morn........	29·983	48	52	NNE		Fine
	Noon........	29·983	58	45	NE	12·195	Cloudy
	Even	29·988	50	54	NE		Fine
21	Morn........	29·981	45	54	NNE		Fine
	Noon........	29·932	52	42	NE to NW	13·065	Fine
	Even	29·912	47	43	NE		Clear
22	Morn........	29·898	43	53	E by N		Fine
	Noon........	29·882	51	43	E by N	8·675	Clear
	Even	29·863	46	53	E		Clear
23	Morn........	29·791	40	84	NNE		Foggy
	Noon........	29·800	48	58	NE	17·771	Cloudy
	Even........	29·773	42	50	E		Clear
24	Morn........	29·820	38	82	NNE		Sma. rain
	Noon........	29·805	49	48	NNE	16·549	Fine
	Even	29·805	44	58	ENE		Cloudy
25	Morn........	29·750	39	65	NE by N		Cloudy
	Noon........	29·747	41	58	NE by N	15·932	Cloudy
	Even	29·720	39	56	NNE		Cloudy
26	Morn........	29·655	40	55	N		Cloudy
	Noon........	29·605	44	52	NW	14·197	Cloudy
	Even	29·550	45	52	NW		Cloudy
27	Morn........	29·542	43	54	NNE		Fine
	Noon........	29·610	47	45	NE	19·58	Cloudy
	Even	29·700	41	46	NNE		Fine
28	Morn........	29·713	44	49	NW		Cloudy
	Noon........	29·700	50	43	WNW	10·191	Cloudy
	Even........	29·665	49	43	W		Cloudy
29	Morn........	29·555	45	53	W by N		Cloudy
	Noon........	29·470	49	44	W	16·166	Cloudy
	Even	29·405	50	48	W by S		Fine
30	Morn........	29·385	44	60	N		Fine
	Noon........	29·325	45	64	NNE	18·953	Showery
	Even	29·380	41	64	N		Showery

ARTICLE IX.

Philosophical Transactions of the Royal Society of London for the Year 1816.

This volume contains the following papers:—

1. *On the Fire-Damp of Coal-Mines, and on Methods of lighting the Mines so as to prevent Explosion.* By Sir H. Davy, LL.D. F.R.S. V.P.R.I.

The author confirmed the experiments of preceding chemists, who had considered fire-damp as carbureted hydrogen. He found it the least combustible of the gases.

2. *An Account of an Invention for giving Light in explosive Mixtures of Fire-Damp in Coal-Mines by consuming the Fire-Damp.* By Sir H. Davy.

This consists of the well-known lamp covered with a wire sieve, a very ingenious invention, which has contributed so much to extend our ideas respecting the combustion of gases, and the explosion of gaseous mixtures.

3. *On the Developement of Exponential Functions, together with several new Theorems relating to Finite Differences.* By John Frederick W. Herschel, Esq. F.R.S.

As it would be impossible to give an intelligible abridgment of this important paper, I must refer those who are desirous of understanding it to the volume of the Transactions itself.

4. *On new Properties of Heat, as exhibited in its Propagation along Plates of Glass.* By David Brewster, LL.D. F.R.S. Lond. and Edin.

When a plate of glass is laid with its edge upon a bar of red-hot iron placed horizontally, and a ray of light polarized in a plane inclined 45° to the horizon is transmitted through it, the light will be polarized in various degrees in different parts of the glass. The glass in fact acquires a crystalline structure, which changes its character with the temperature, and which vanishes when the heat is uniformly diffused over the plate. The edge of the glass lying on the hot iron and the opposite edge acquire the same structure as that class of doubly refracting crystals (quartz, selenite, &c.) in which the extraordinary ray is attracted to the axis, while the centre of the plate has the same structure as the other class of doubly refracting crystals (calcareous spar, beryl, &c.) in which the extraordinary ray is repelled from the axis. Between the centre and each of the edges there is an intermediate space, which has a structure similar to that of common salt, fluor spar, &c. bodies destitute of double refraction. These phenomena, and many others depending on them, which are described in this curious paper, are of the most fugitive nature. But Dr. Brewster has discovered a method of rendering them permanent, and consequently of subjecting the phenomena to measurement. When a plate of glass is heated red-hot, and cooled in the open air, or when one of its edges is placed upon a bar of cold iron, the same appearances are developed during the cooling of the glass as were exhibited in the preceding case during its heating; and when the glass is cold, the structure producing the fringes remains permanent.

Dr. Brewster has shown that these changes on the structure of glass are independent of changes in its temperature, and that they are analogous to the phenomena of electricity and magnetism. The fact of the crystalline structure given to glass by suddenly cooling it had been discovered by Dr. Seebeck, and published in Schweigger's Journal, vol. xii. p. 1. I may take this opportunity of informing Dr. Brewster that my copy of this journal is not the only copy of it in Great Britain. There is a copy of it in the College Library of Edinburgh; and I know from some circumstances that the number in question was in the Edinburgh Library some months before Dr. Brewster's paper was read to the Royal Society.

5. *Further Experiments on the Combustion of explosive Mixtures confined by Wire-Gauze, with some Observations on Flame.* By Sir H. Davy.

This paper contains a number of experiments determining the proper size of the meshes of the wire-gauze proper for the safe lamp. It contains, likewise, the first attempt to account for the fact that wire-gauze prevents explosions from taking place when a lamp is burned in an exploding mixture. Sir H. ascribes it entirely to the cooling power of the wire-gauze. From subsequent facts which the author has since ascertained, there is reason to believe that this explanation, though at first sight rather paradoxical, is the true one.

6. *Some Observations and Experiments made on the Torpedo of the Cape of Good Hope in the Year* 1812. By John T. Todd, late Surgeon of his Majesty's Ship Lion.

When the Lion was at the Cape of Good Hope, torpedos were frequently caught by the seine, which put it in the power of Mr. Todd to make some observations on them. They were always small, never exceeding eight inches in length. The electrical organs were cylindrical, and were supplied with more nerves than any other part of the body. The shocks were perfectly voluntary on the part of the animal. Those animals that gave numerous shocks were soon exhausted, and died; while those that refused to give shocks continued to live much longer. When the nerves of the electric organs were cut, the animal lost the power of giving shocks, but the length of its life was not diminished.

7. *Direct and expeditious Methods of calculating the eccentric from the mean Anomaly of a Planet.* By the Rev. Abram Robertson, D.D. F.R.S. Savilian Professor of Astronomy in the University of Oxford, and Radcliffian Observer.

8. *Demonstration of the late Dr. Maskelyne's Formulæ for finding the Longitude and Latitude of a celestial Object from its right

Ascension and Declination, the Obliquity of the Ecliptic being given in both Cases. By the Rev. Abram Robertson, D.D.

9. *Some Account of the Feet of those Animals whose progressive Motion can be carried on in Opposition to Gravity.* By Sir Everard Home, Bart. V.P.R.S.

The common house fly, it is well known, can walk with facility up the perpendicular surface of panes of window glass, and even upon the ceiling of the room, thus supporting itself contrary to gravity. But the foot of this animal is so small, that its anatomical structure cannot be ascertained. But the lacerta gecko, a native of Java, possesses a similar power. It is an animal of considerable size, weighing above 5 oz. The author obtained a specimen of this animal from Sir Joseph Bankes, and was enabled in consequence to ascertain the structure of its foot. Each foot has five toes, which terminate each in a crooked claw. Round the toe there are a set of transverse openings or pockets with serrated edges. When these serrated edges attach themselves to the wall, the pockets are extended by a set of muscles adapted for the purpose. A vacuum of course is formed in each. The consequent pressure of the air is sufficient to keep the foot attached to the wall, and to support the weight of the animal. The structure of the top of the head of the echineis remora, or sucking fish, is similar. By means of it the animal keeps itself attached to the shark, or to the bottom of ships. There can be no doubt that the structure of the feet of flies must be similar.

10. *On the Communication of the Structure of doubly refracting Crystals to Glass, Muriate of Soda, Fluor Spar, and other Substances, by mechanical Compression and Dilatation.* By Dr. Brewster.

When the edges of a plate of glass are pressed together by any kind of force, it exhibits distinct neutral and depolarizing axes, like all doubly refracting crystals, and separates polarized light into its complementary colours. The neutral axes are parallel and perpendicular to the direction in which the force is applied, and the depolarizing axes are inclined to these at angles of 45°. When a plate of glass is bent by the hand, one side of it is compressed, and the other dilated. The compressed side has a structure the same as that of calcareous spar, beryl, &c. while the dilated side has a structure similar to that of quartz, sulphate of lime, &c.

Common salt, fluor spar, and other similar bodies, acquire the same structure by compression and dilatation. But compression and dilatation produce no change in the structure of those bodies that already possess the property of refracting doubly.

Compression and dilatation produce the same effects upon animal jelly as upon glass.

11. *An Essay towards the Calculus of Functions.* Part II. By Charles Babbage, Esq.

12. *Experiments and Observations to prove that the beneficial Effects of many Medicines are produced through the Medium of*

the circulating Blood, more particularly that of the Colchicum Autumnale upon Gout. By Sir Everard Home, Bart.

It is well known that mercury produces the same effects on the system, whether it be introduced through the absorbents, or by the stomach. The author made an experiment with a dog to ascertain whether this was the case likewise with the *eau medicinale.* He introduced a certain quantity of this substance into the circulation of a dog through the jugular vein. He made the dog afterwards swallow a quantity of the same medicine. The effects in both cases were the same.

13. *Appendix to the preceding Paper.* By Sir Everard Home, Bart.

In this appendix he gives the account of the effects of the introduction of a large quantity of eau medicinale into the circulation of a dog. It produced all the symptoms induced by swallowing an over dose of the medicine, and occasioned death.

14. *On the cutting Diamond.* By W. H. Wollaston, M.D. Sec. R. S.

The diamonds chosen for cutting are all crystallized. The surfaces are curved; and hence the meeting of any two of them presents a curvilinear edge. If the diamond be so placed that the line of the intended cut is a tangent to this edge near its extremity, and if the two surfaces of the diamond laterally adjacent be equally inclined to the surface of the glass, then the conditions necessary for effecting the cut are complied with. A simple fissure is effected which need not be more than $\frac{1}{200}$th of an inch in depth. When a force is applied at one end of this fissure, a crack extends itself almost certainly in the direction of the fissure. Dr. Wollaston found that other bodies, as sapphyr, ruby, spinell, when ground into the same curve surfaces as the diamond, would also cut glass; but the edges very speedily lost the requisite shape.

15. *An Account of the Discovery of a Mass of native Iron in Brasil.* By A. F. Mornay, Esq.

This mass was found in about 10° 20′ S. lat., and about 33′ 15″ long. W. from Bahia. It had been discovered in 1784, and an unsuccessful attempt made to bring it to Bahia. It is about seven feet long, four feet wide, and two feet thick; but of an irregular shape. Mr. Mornay calculates its solid contents at 28 cubic feet, and its weight about 14,000 lb. Its upper surface is glossy, and chesnut-coloured; being covered with a thin coat of rust, the under surface is scaly.

16. *Observations and Experiments on the Mass of native Iron found in Brazil.* By Dr. Wollaston.

The specimen exhibited a crystalline texture, and was disposed to break in octahedrons, tetrahedrons, or the rhomboids formed by their junction. It was magnetic by induction, like common iron, It was composed of

Iron	96
Nickel	4
	100

Dr. Wollaston's mode of detecting nickel in iron is this. He dissolves a minute portion of the metal in nitric acid, evaporates the solution to dryness, lets fall a drop of ammonia on the dry mass, heats it gently to dissolve the oxide of nickel, draws the liquid to a little distance from the oxide of iron, and then adds some triple prussiate of potash. The appearance of a milky cloud indicates the presence of nickel. To determine the quantity of nickel he converts it into sulphate of nickel: 10 gr. of nickel form 44 gr. of sulphate of nickel.

17. *On Ice found in the Bottom of Rivers.* By T. A. Knight, Esq. F.R.S.

Mr. Knight observed upon a mill-pond in the river Teme, in Herefordshire, millions of little frozen spiculæ floating on the surface of the water. At the end of this pond the water fell over a low weir, and entered a narrow channel, where its course was obstructed by points of rocks and large stones. By these numerous eddies were occasioned, which drew the floating particles of ice under water. These particles striking against the stones at the bottom of the river, adhered to them; and in this way a quantity of ice accumulated at the bottom of the river; and had the cold continued, it would no doubt have covered the whole bottom.

18. *On the Action of the detached Leaves of Plants.* By T. A. Knight, Esq.

Mr. Knight had formerly given it as his opinion that the matter which becomes vitally united to trees previously passes through their leaves. The object of this paper is to state fresh evidence in proof of this opinion. Pieces of bark separated from the branch of a vine, and attached only to the foot stalk of a leaf, continued to vegetate, and to increase in bulk, as if they had been attached to the tree. Leaves of the potatoe planted in pots, and regularly watered, continued to vegetate till winter; and when pulled up, the bottom of the foot stalk had swelled out, and consisted of matter similar to the tubers of the potatoe. A branch of the vine being cut off, and laid horizontally, with part of each mature leaf dipping into a bason of water, the immature leaves, and the extremity of the branch continued to grow and elongate.

19. *On the Manufacture of the Sulphate of Magnesia at Monte della Guardia, near Genoa.* By H. Holland, M.D. F.R.S.

The mountain of Guardia is composed of clay-slate, over which lie beds of serpentine and magnesian lime-stone, in which occur veins of iron and copper pyrites, obviously mixed with matter containing magnesia. A manufacture was originally established to extract blue and green vitriol from these pyrites; but the appearance of sulphate of magnesia during their processes induced the proprietors to convert it into a sulphate of magnesia manufactory. The pyrites is roasted, and then left in a shade for some time, being occasionally moistened with water. It is then lixiviated. The copper is thrown down by means of iron, and the iron by means of magnesian lime-stone. The liquid is then filtered and concentrated sufficiently for the crystallization of the sulphate of magnesia.

20. *On the Formation of Fat in the Intestines of the Tadpole, and on the Use of the Yelk in the Formation of the Embryo in the Egg.* By Sir E. Home, Bart.

The length of intestine in the tadpole, when compared with that of the animal, is greater than in any other creature. During this state a quantity of fat is deposited on the loins of the tadpole. When the tadpole is changed into a frog, the intestine becomes much shorter, and the fat has disappeared. The author conceives that the use of the great length of intestine in these animals was the formation of fat, and that the fat was deposited to assist in the subsequent transformations of the animal. The ovum of the frog contains no yolk.

21. *On the Structure of the Crystalline Lens in Fishes and Quadrupeds, as ascertained by its Action on polarized Light.* By Dr. Brewster.

The author concludes from his experiments that the central nucleus and the external coat of the lens are in a state of dilatation, while the intermediate coats are in a state of contraction.

22. *Some further Account of the Fossil Remains of an Animal of which a Description was given to the Society in* 1814. By Sir E. Home, Bart.

From specimens in the possession of the Rev. Mr. Buckland and Mr. Johnson, the author considers it as established that the animal in question was a fish, but quite different in its structure from any known species.

23. *Further Observations on the Feet of Animals whose progressive Motion can be carried on against Gravity.* By Sir E. Home, Bart.

The author gives further examples of the structure described in his former paper.

24. *A new Demonstration of the Binomial Theorem.* By Thomas Knight, Esq.

25. *On the Fluents of Irrational Functions.* By Edward Ffrench Bromhead, Esq. M.A.

ARTICLE X.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, May 1, a paper by Sir Everard Home, on the passage of the ovum from the ovarium into the uterus, was read. Very little light has hitherto been thrown on this subject. Hervey, though supplied with deer by royal munificence, failed in his investigations. John Hunter was equally unsuccessful with sheep. Accident threw into the author's way an observation which serves to cast a new light upon this obscure subject. A female servant, aged 23, was absent from home about four hours, and returned in high spirits. She fell ill in the evening, had an epileptic fit and fever, and died in a week. On examining the body after death, the uterus gave signs of having been impregnated. She had been impregnated a week before death. The ovum was in the uterus, enveloped in coagulated lymph; but Mr. Bower was able, by his skill in using the microscope, to examine it, and to determine its nature unequivocally. It had come from the ovarium on the left side, which was of a larger size than the other. Two corpora lutea were observable, and there were several cavities from which ova had previously made their escape. Sir Everard conceives that these ova make their escape occasionally, whenever any great excitement of the system takes place. The semen of the male makes its way to the uterus, and the impregnation takes place there. He conceives that the ovum remains in contact with the male semen for several days to complete the impregnation.

On Thursday, May 8, a paper by Sir Everard Home was read, on a method of rendering the use of the colchicum autumnale as a medicine for the gout much milder. The author related a set of experiments to show that the decoction of the colchicum acts precisely in the same way as the eau medicinale; from which he concludes that they are the same medicine. When the infusion of colchicum is kept, it lets fall a sediment, which Sir Everard found to act violently as a purgative. When this sediment is separated, the medicine acts much more mildly, though its specific effect on the gout is still the same. Hence he conceives that by removing this sediment the medicine is rendered much milder in its action without injuring its beneficial effects.

At the same meeting a paper by Thomas A. Knight, Esq. was read, on the expansion of the wood of trees in different directions. The author had suggested in a former paper the probability that the ascent of the sap in trees was occasioned by the action of what is called the silver grain of the wood. The object of the present paper is to confirm that opinion. If a horizontal section of a new felled tree be sawed in the direction from the bark to the pith, it speedily expands so much as to catch hold of the saw and prevent its action. If the parts be kept asunder by a wedge, and the sawing continued to the centre of the tree, the instant the wedge is removed the two sides of the cut close together with violence. If another slit be cut in the section from the bark to the centre at a short distance, so as to detach a slip of the tree altogether from the rest, this slip does not fall out, but is retained in its place by the expansion of the wood. If the section be sawed in any other direction, so as to cut across the fibres of the silver grain, instead of merely separating them, no expansion takes place, and the saw continues to act freely. The pith in trees has a larger diameter when the tree is full of sap than when dry. He took branches, dried them well, and then forced in pieces of metal, so as to fill exactly the space occupied by the pith. The pieces of wood were then buried in moist earth, so as to absorb moisture. When in this state, the pieces of metal had become so loose, that they dropped out of themselves.

On Thursday, May 15, part of a paper by Dr. John Davy, on the Temperature and Specific Gravity of the Sea, and on the Temperature of the Air over the Sea in Tropical Regions, was read. The observations contained in this paper were made by the author during his voyage from England to Ceylon. The temperature of the air was marked every two hours, both night and day. The sea water, when drawn up, was tried by means of a thermometer to ascertain its temperature, and then weighed in a weighing bottle capable of containing about 300 grains. During the latter part of the voyage the sea water was corked up in phials, and its specific gravity determined after he landed in Ceylon. It was taken at the temperature of 80°, which is nearly the mean heat of the tropical countries. The general result of these observations is, that the specific gravity of the sea is nearly the same every where. He does not agree with a modern traveller of high authority, who considers the specific gravity of sea water to differ in every zone. The small differences that exist are not easily accounted for. In one case he found the specific gravity diminished after very heavy rain. It was generally altered by squally weather. In general the temperature of the air was highest exactly at noon, and lowest just at sun-rise; but in a perfect calm the temperature of the air was the same as on land; namely, its greatest height was some time after noon. The reason is, that heat accumulates both in the ship and in the sea.

LINNÆAN SOCIETY.

On Tuesday, May 6, a paper by Andrew Knight, Esq. on the Species of the common Strawberry, was read. The author is of opinion that no plants can be considered as constituting different species, excepting those incapable of propagating with each other. He therefore planted all the different varieties of strawberry known in this country in garden pots, and cultivated them in the proper situation to impregnate one another, and continued his experiments for several years. The result was, that there are only three distinct species of strawberry known in this country, though some of them assume many various appearances.

At the same meeting, a description of some fossil bones found on the coast of Norfolk, by Dr. Arnold, was read. The bones in question had some resemblance to those of the turkey; but the author of this paper did not attempt to make them out.

At the same meeting, some further observations on alcyonia, by Dr. Arnold, were read.

GEOLOGICAL SOCIETY.

Nov. 15, 1816.—A paper by W. H. Gilby, M.D. of Bristol, on the Magnesian Lime-stone and the red Marl of the Neighbourhood of Bristol, was read.

The strata in the neighbourhood of Bristol may be distinguished into two classes. The older of these comprehends the old red sandstone, the first floetz or mountain lime-stone, and the coal formation. The beds of the two former are highly inclined, and enclose within their lines of basset irregularly elliptical areas, towards the interior of which they dip on all sides. The coal formation fills up these areas, the lower beds of which at least dip conformably with those of the lime-stone on which they rest, and at an equal angle. The second, or newer class of strata, is horizontal, or nearly so, in its position, and lies unconformably on the tilted edges of the strata first mentioned. It is composed of the various beds which form that extensive and important deposit which is generally known by the name of red ground or red marl. Of these beds the lowest is a conglomerate of fragments of common lime-stone cemented by ferruginous sand, above which are beds of red and white calcareous sand-stone, and then a deposit of red clay containing gypsum and sulphate of strontites. At Portishead, a village on the Bristol channel, the conglomerate makes its appearance, but exhibiting some peculiarities in its external characters; the basis, in particular, being of a yellow colour, and resembling the Yorkshire magnesian lime-stone. This circumstance induced Dr. G. to make a chemical analysis of it, which ascertained it to contain 37·5 per cent. of carbonate of magnesia. It may be traced along the shore from the village just mentioned to Clevedon, every where containing the same fragments, and lying in horizontal beds on the inclined strata of old red sand-stone. It is, therefore, in Dr. G.'s opinion, to be regarded as a mere variety of the common lime-stone conglomerate, and as occupying a geological situation precisely similar to the magnesian lime-stone of the north of England. Dr. Gilby also mentions that magnesian lime-stone sometimes occurs in beds interstratified with the mountain lime-stone, 1. At Ross, in Herefordshire, where it assumes the appearance of dolomite. 2. About four miles north-west of Bristol, where it abounds in shells, entrochi, and madrepores.

A notice from Mr. Sowerby was read on some Fossil Organic Remains found on the banks of the Tagus, near Lisbon.

These remains are an ostrea similar to the ostrea virginia, Linn.

a mactra, the cast of a large serpula, and other shells which bear a great resemblance to those found in the calcaire grossiere of Paris.

The reading of Dr. Berger's paper, entitled, Geognostic Remarks on the Rocks in the immediate Vicinity of Dublin, was concluded.

The oldest rock in the immediate vicinity of Dublin is greywacke. Nearly the entire promontory of Howth is composed of this rock in all its varieties, interstratified with subordinate beds of clay-slate. The mean dip of the beds is about south-east, at an angle of 38°. Besides the clay-slate, other subordinate beds occur in this formation, such as flinty-slate in the island called Ireland's Eye, and compact porphyritic felspar on Ratheoote Common. Of the floetz rocks, the oldest is a shell lime-stone, from which all the lime in the neighbourhood of Dublin is procured. It is chiefly characterized by encrinital remains and the anomia producta. It dips SE by S, at a mean angle of 45°. A magnesian lime-stone alternating with beds of shelly and clayey lime-stone appears to rest on the encrinital lime-stone. It dips nearly SW, at an angle of about 27°. The Calp lime-stone, or *building-stone*, is the next in succession, and occupies by far the greatest part of the district here described. It consists of many beds, and contains the five following varieties of rock. 1. *Building-stone*, in beds of from 18 inches to 2½ feet thick, and of remarkably regular stratification. Its colour is grey, approaching to black. When rubbed, it gives an odour of sulphureted hydrogen. It burns white, but does not form a good quicklime, nor does it contain any organic remains. 2. *Flags*. These are beds more or less slaty, which intervene between the beds of building-stone. They are of a more earthy texture than the latter, and contain spangles of mica. Their thickness varies from three inches to one foot. 3. *Lime-stone*, in beds rarely less than three feet thick. It contains no organic remains; and, not being recognized by the quarrymen as a lime-stone, is made no use of. 4. *Wallers*, or *ashlers*. The beds to which this name is given are of an extremely dense close texture, and of a blue colour. 5. *Black flint*, or *chert*, in continuous layers, one or two inches thick. The general dip of the Calp formation is nearly due S. at an angle of 19°.

Dec. 6.—The reading of a paper from Mr. Phillips, entitled, On the Forms and Measurements by the reflecting Goniometer of certain primitive Crystals, with Observations on the Method of obtaining them by mechanical Division, was begun.

Dec. 20.—At this meeting the reading of Mr. Phillips's paper was concluded.

The substances noticed in this paper are oxide of tin, sulphate of barytes, quartz, zircon, staurotite, anataze, specular iron, diopside, cyanite, corundum, sulphate of strontian, carbonate of lead, sulphate of lead, blue carbonate of copper.

The crystallographical history of the two former of these substances having been already communicated to the Society, they are introduced into the present paper only for the purpose of describing the best methods of obtaining sections of them in the direction of their natural joints. Of the other substances, not only the best methods of cleaving them are pointed out, but the results of their measurement by the reflecting goniometer are stated, and compared with those which have been obtained in the usual way by Bournon and Haüy.

A supplementary notice on the Quartz Rock of Sky, by the President, was read.

The rock in question forms a large mass of erect strata alternating with red sand-stone and greywacke schist.

The latter strata extend in a north-east direction from one shore of the island to the other; but the quartz rock accompanies them only for about five miles. The structure of this quartz is for the most part compact, with a splintery fracture: occasionally it becomes more or less granular, and now and then contains grains of felspar.

Jan. 3, 1817.—At this meeting the reading of a paper by the President on the parallel Roads of Glenroy, was begun.

Jan. 17.—At this meeting the reading of Dr. Macculloch's paper was concluded.

A long valley extends from the skirts of Ben Nevis to the mouth of the Spey, and is divided into two unequal portions by a low boggy hill of granite, that forms its summit level. On the south side of this hill is the source of the Spey, which flows to the south-east; and on the north side is the source of the Roy, the waters of which flow north-west into the great Caledonian valley extending from Fort George to Fort William. On the sides of Glenroy, and of some of the lateral valleys, are traced strong lines parallel to each other and to the horizon. The two corresponding lines on each side of the valley coinciding precisely in level and elevation with each other.

These lines have been attributed to various causes. By some they have been considered as the work of man; and by others as the effect of natural agents.

Those who adopt the latter hypothesis agree in ascribing them to the action of water, on account of the perfect levelness of the lines, and their parallelism to each other and to the horizon; but they differ from one another in this respect, one party attributing them to the wearing of a torrent or current of the sea; and the other conceiving that the hypothesis of their having been the shores of a lake is better adapted to explain the present appearances.

Into the consideration of all these hypotheses the author of this paper enters with much minuteness; and though he considers the latter as the more probable theory, yet allows and states at large the difficulties which attend every mode of accounting for these remarkable phenomena.

Feb. 21.—At this meeting a paper by George Cumberland, Esq.

Hon. Mem. G. S. entitled, "Description of the newly-discovered Heads of Encrinites, of which 17 Drawings are sent for Exhibition, and a Sketch of the District wherein they are found, was read.

A stratum of encrinital lime-stone from 20 to 40 feet thick is seen cropping out in a line from the Black Rock, near Bristol, to the shore of Clevedon Bay, and at the back of a tongue of land called Woodspring Point. Throughout this rock remains of stems of encrinite are abundant, together with other marine exuviæ; but till lately no remains of heads were found. After long research, however, by Mr. Cumberland, and other gentlemen of the neighbourhood, some of these parts of the animal were discovered, and investigation proves they are of several species. Among them, as appears by the drawings, is the nave encrinus of Parkinson; but the greater number are of species hitherto unknown.

A paper by Arthur Aikin, Esq. M.G.S. was read, entitled, Some Observations on a Series of Specimens from Torre del Greco presented to the Geological Society by the Hon. H. G. Bennet.

On June 15, 1794, part of the town of Torre del Greco was overwhelmed and buried by a stream of lava from Mount Vesuvius. About twelve months afterwards the lava had considerably cooled, the heat indicated by a thermometer placed in the crevices being 178° Fahrenheit, and new buildings were erecting on it. In digging the foundations of these new houses, the ruins of those that had been covered by the lava were occasionally broken into, and several articles were obtained which had during a year been subjected to the heat of the torrent. Many interesting specimens of these were obtained by the Hon. H. G. Bennet, who has presented them to the Geological Society; and Mr. Aikin, in this paper, has given his observations upon them.

Several pieces of glass, which appear to have been acted on in various degrees by the heat, show changes similar to those produced in the laboratory by burying them in red-hot sand, excepting that this slower process has produced the more crystalline structure. Those which have actually undergone fusion have become masses more or less cellular, differing but little in structure and general appearance from ordinary glass. These changes coincide with the results obtained by Reaumur in his experiments.

Pieces of iron have become converted into the state of black, red, grey, and magnetic oxide, and having in the hollows and interstices crystals of brownish-red transparent oxide of iron, and of specular iron ore. From the changes that the various articles of iron have undergone, the author concludes that there was little, if any, free sulphur in the lava, since there is no appearance of iron pyrites. Also, as the forms of the articles have not been materially altered, and the crystals are in many instances produced by sublimation, that iron or its oxide becomes volatile at a much lower temperature than has hitherto been observed.

Pieces of copper show changes into the states of crystallized red oxide, and red oxide mixed with green and blue carbonate.

Lead has become oxidated, and some small pieces are intermixed in a mass of it which appear to be galena. The sulphur in this case the author considers to be obtained by the depuration of the lead from long exposure to heat, the uncommon softness of the metallic lead being a circumstance to support the conjecture.

There are also specimens of compact minium derived from common shot.

March 7, 21.—At these meetings a paper by Dr. Macculloch, entitled, Corrections and Additions to the Sketch of the Mineralogy of Sky published in Vol. III. of the Transactions of the Geological Society, was read.

In a visit to the island in the course of last summer Dr. Macculloch was enabled to continue his examination of its structure, and to detect some errors which had occurred in the former paper, occasioned partly by the inaccuracy of Mackenzie's chart, which was his guide in his first journey.

In the paper already before the Society the promontory of Sleat was stated to consist of micaceous schist as the lowest rock of the island. To this succeeded greywacke and schist, then quartz rock, and afterwards red sand-stone. In the present examination Dr. Macculloch has found that the beds of gneiss alternate with and are in greater proportion than the mica-slate. This gneiss passes into a rock composed of felspar and quartz, with chlorite schist interlaminated together, the latter being substituted for the mica of the regular varieties. Decided alternations of the various rocks connect the red sand-stone with these beds, and offer an extraordinary instance of the connexion of the red sand-stone with a primary rock—the gneiss.

Among the lyas lime-stone beds with shales and sand-stones intervening which lie over the red sand-stone, changes are met with that have converted the lime-stone into marble, and the sand-stone into quartz. On the western shore of the northern district the lyas is converted into chert, and the shale into siliceous schist. These changes can be traced through various intermediate states.

The rocks of Trotternish are those of the lyas formation intersected and covered by trap. In many places the trap appears interstratified with the beds below it. Often the alternations of the trap are as regular as those of the stratified rocks with which it is connected; but it invariably happens that after some distance the trap which has continued between two of the beds passes through the one or below, either uniting with the superincumbent mass of trap, or passing for a further distance with similar regularity between two other strata.

In some instances Dr. Macculloch has observed this to happen after the trap had continued in a regular course between two beds for more than a mile; and he concludes that all the supposed cases of alternations of the trap rocks with stratified ones are of a similar nature.

The trap of Sky is generally amorphous; but at the northern end of the promontory of Trotternish it is columnar on a large scale. The columns are 200 or 300 feet high; yet they are equal to those of Staffa in symmetry and beauty.

The author considers this trap to be generally composed of a rock analogous to green-stone, in which augit occupies the place of hornblende. This rock is of frequent occurrence in Scotland, and Dr. Macculloch proposes to call it augit rock.

The Cuchullin hills consist of another member of the trap family, composed of hypersthene and felspar, to which the author gives the name of hypersthene rock.

Dr. Macculloch concludes his paper with the account of an alluvium of which it is difficult to explain the origin. This is found near Killchaken, opposite to the main land of Scotland, occupying a space of about a mile in length, and a few hundred yards in breadth; and its surface is 60 or 70 feet above the level of the sea. There is no appearance of rivers having ever flowed near this plain, and the uniformly level surface of the deposit, and its elevation, are obstacles to the supposition of its being derived from the rejection by the sea of the rolled fragments of the surrounding mountains. The bar of Killchaken Harbour, and the gravelly soundings of the shore, are indications of its extent having once been more considerable; and render it probable that this is the remains of some ancient diluvian deposit, which perhaps in former times may have united the island to the opposite coast. Instances of similar deposits, though rare in the islands, are of frequent occurrence in many parts of Scotland.

ROYAL ACADEMY OF SCIENCES.

Analysis of the Labours of the Royal Academy of Sciences of the Institute of France during the Year 1816.

Physical Part.—By M. le Chevalier Cuvier, Perpetual Secretary.

While restoring to the Class of Sciences of the Institute a name rendered illustrious by more than a century of useful labours, while allowing them to associate with persons who, without making the sciences their habitual profession, consider it as an honour to be acquainted with them and to serve them, the King has condescended to preserve to that company the organization which it has recently received, and of which a sufficiently long experience has demonstrated the advantages. The Academicians, exempted at their entrance from all dependance, and from all humiliation, and not afraid of seeing that union altered which a common love of study so naturally maintains, will continue each to cultivate with zeal that portion of the great scientific domain which he has selected, and to submit to the judgment of his associates the fruits which he has collected. Our analyses of course, as well as their labours, will retain their old form. The one which we offer at present to the public will unite without interruption to the preceding ones.

Let us hope that Peace, the communications which it opens, and the emulation which it excites, will contribute to render the contents of our analyses more and more interesting.

PHYSICS AND CHEMISTRY.

It is well known that the different bodies, and particularly the different liquids, are dilated by heat in very different proportions.

M. Gay-Lussac has endeavoured to discover some law which should point out the rule of these proportions. For this purpose, instead of comparing the dilatations of different liquids above or below a uniform temperature for all, he set out from a point variable in point of temperature, but uniform as far as regards the cohesion of the molecules; namely, from the point at which each liquid boils under a given pressure; and among those which he examined he found two which dilate equally from that point. These are alcohol and sulphuret of carbon; which boil, the former at 173·14°, the latter at 115·9°. The other liquids did not present in this respect the same resemblance. On inquiry into the other analogies of the two liquids in question, M. Gay-Lussac ascertained that they resemble each other likewise in this respect, that the same volume of each at its boiling point gives under the same pressure the same volume of vapour; or, in other terms, that the densities of their vapours are to each other as those of the liquids at their respective boiling temperatures.

M. Gay-Lussac promises to prosecute his researches, and to present shortly a more complete set of experiments on the dilatation of liquids and their capacity of heat, compared with the capacities of their vapours.

Among the delicate questions with which chemistry is at present occupied, we ought to place that which regards the proportions according to which the elements are capable of uniting to form the different kinds of compounds. It has been lately observed that there are certain limits which nature affects, expressed by terms generally simple; and, according to the researches of Gay-Lussac, this is the case particularly with the combinations of the gases when we regard not their absolute weight, but their volume under an equal pressure.

These researches are liable to great difficulties, because it is not always possible to obtain the combinations isolated; and when we wish to separate them from the salts of which they constitute a part, they are decomposed or altered by the other principles of these salts, or by the water which almost always enters into them.

In this way we may explain the striking differences in the results of Davy, Dalton, and Gay-Lussac, respecting the combinations of azote and oxygen.

From the experiments presented during this year to the Academy by Gay-Lussac, it follows that nitrous gas contains a volume of azote and an equal volume of oxygen without condensation; that in certain circumstances there combines a volume of azote with a volume and a half of oxygen, to which Gay-Lussac gives the name of *pernitrous acid;* that common nitrous acid is composed of one volume of azote united to two volumes of oxygen; and that nitric acid is composed of one volume of azote united to two volumes and a half of oxygen.

Among these different varieties (if we may so express ourselves) of oxides or acids which have azote for their radical, there is one obtained by the distillation of neutral nitrate of lead previously dried. It is a very volatile liquid, of an orange colour. Gay-Lussac considered it as nitrous acid, the elements of which were kept united by means of a quantity of water which constituted a part of it. But M. Dulong has ascertained by very exact analyses that it contains no water, and on that account has given it the name of anhydrous nitrous acid. His result has been confirmed by synthesis. One volume of nitrous gas, and a little more than two volumes of oxygen gas, exposed to an artificial cold of — 4°, forms this acid, which, among other properties, changes colour, not only by being mixed with water, but likewise by heat. At — 4° it is colourless, at 59° it becomes orange, and at 82° almost red. Four parts of nitrous gas and one part of oxygen gas, condensed in the same way by cold, formed a deep green liquid, much more volatile than the preceding liquid, which M. Dulong considers as a simple mixture of nitrous acid and another acid in which the proportion of nitrous gas is much greater.

M. Dulong has examined likewise the proportions in which oxygen combines with phosphorus to form acids. Before him only two acids had been admitted. His researches have induced him to believe that there are four. That which contains the least oxygen is obtained by throwing an alkaline phosphuret into water. Phosphureted hydrogen is disengaged, and the oxygen of the water forms with the remaining phosphorus an acid which remains combined with the alkali, and which may be separated by sulphuric acid. M. Dulong calls it *hypo-phosphorous acid.* But he is of opinion that hydrogen enters into it as a constituent.

A second acid, to which M. Dulong gives the name of *phosphorous,* is obtained by decomposition of water when proto-chloride of phosphorus is put into that liquid. Two acids are formed, namely, the muriatic and this of which we are speaking. M. Dulong is of opinion that it is a compound of 100 parts of phosphorus and 75 of oxygen.

The third acid is that which is produced by the slow combustion of phosphorus in the air. It is decomposed when saturated into phosphoric and phosphorous acids, and gives at the same time phosphites which are more soluble, and phosphates which are less so. However, M. Dulong does not regard it as a simple mixture, but rather as a combination of these two acids, having some resemblance to saline compounds, and in which the phosphorous acid acts the part of the base. On this account he proposes to call it *phosphatic acid*, in order to recall the analogy which it has with the phosphates.

The last term of the oxygenation is the phosphoric acid. The proportion of the phosphorus to the oxygen in it is as 100 to 124. It is obtained by the rapid combustion of phosphorus, or by the decomposition of water by the bichloride of phosphorus, and by various other processes. It is identical with that which is obtained from the bones of animals.

Three Dutch chemists, MM. Van Marum, Dieman, and Paëts Van Troostwick, made known in 1796 a gas composed of carbon and hydrogen, which they called olefiant gas, because its most singular property was that of forming an oily liquid when mixed with oxymuriatic acid gas. From the theory of oxymuriatic acid at that time prevalent, the natural opinion was that its oxygen uniting with the olefiant gas constituted the oily liquid in question; but at present, when this gas is considered as a simple substance, to which Davy has given the name of chlorine, we are under the necessity of looking out for a different explanation. MM. Robiquet and Colin undertook that investigation. They ascertained that when one volume of olefiant gas and two volumes of chlorine are made to mix slowly in a glass globe they are converted entirely, and without residue, into an oily liquid; which, when decomposed by heat, gives hydrogen not saturated with carbon, a deposite of carbon, and much muriatic acid; that is to say, according to the new theory, chlorine united to hydrogen. Hence it follows that chlorine enters entirely into the composition of the oily liquid. But does it enter in the state of chlorine, and unite directly to the carbureted hydrogen? or is it united with hydrogen, and in the state of muriatic acid? The authors have been led to the first of these conclusions by inductions drawn from the specific gravity of the constituents and the compound; while muriatic ether, which has numerous resemblances with this oily liquid, appeared to them, on the contrary, formed by the union of muriatic acid with olefiant gas.

M. Chevreul still continues to labour with the same zeal at his chemical history of fat bodies. We have described after him formerly that hog's lard is composed of two principles—one more consistent, the other more liquid; that the action of the alkalies alters the combination, separates a new principle analogous to Scheele's sweet principle of oils, and occasions the formation of two new principles of an acid nature, with which the alkali combines in order to form soap. We have explained the different affinities of the alkalies and earths for these two acids, and the capacities of saturation of the acids. Lastly, we have given an account of the comparative examination made by Chevreul of the different bodies more or less analogous to *fat;* such as the biliary calculus, spermaceti, and the adipocire of dead bodies, and of the essential differences which characterize them. In a memoir presented to the

Academy during the present year, this laborious chemist has begun to examine the causes to which the consistence, the odours, and the colours, of certain oils and fatty bodies are to be ascribed. He made experiments on the fat of men, oxen, sheep, the jaguar, and the goose. The differences in consistence depend upon the proportion of the two general principles of fatty bodies; but the other differences depend upon peculiar and foreign bodies. M. Chevreul proposes a system of nomenclature analogous to the rest of the chemical nomenclature, both for the principles which he has discovered, and for their saline combinations. The two fatty principles he calls *steatine* and *elaine*, from the Greek words which signify tallow and oil. His most solid acid principle, or his margarine, is *margaric acid*, the other is *elaic acid*. Spermaceti gets the name of *cetine*, &c. These names are no doubt burdensome to the memory; but this is an inconvenience inseparable from the progress of science; and periphrases, which would lengthen discourse without making it more clear, would be attended with inconveniences still more formidable.

(To be continued.)

ARTICLE XI.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Dr. Davis will commence his Spring Course of Lectures on the Theory and Practice of Midwifery, and on the Diseases of Women and Children, on Tuesday, May 20, at Mr. Taunton's Theatre, 87, Hatton Garden.

Mr. A. T. Thomson commenced his Course of Lectures on General and Medical Botany, on Thursday, May 29, at two o'clock, in the Anatomical Theatre, Blenheim-street. It is intended, in this course, to combine instructions on the general principles of physiological botany, and on the classification and systematic arrangement of plants, in which particular attention will be given to select as specimens those plants which have been adopted into the British pharmacopœias. Two distinct Lectures will also be set apart for the consideration and demonstration of plants which are generally regarded as hurtful to the animal economy, or are positively of a poisonous nature. A Lecture will be delivered every Monday, Thursday, and Saturday, until the course be completed.

II. *Detail of Experiments relative to the Prevention of Explosion in the Oxy-hydrogen Blow-pipe.* By Mr. Gray.

The lamp which Sir H. Davy has invented for the prevention of explosion in the coal-mines presents to our view this fact, that though the inflammable gas comes into actual contact with the flame, and is kindled by it, yet the extension of the kindled air is infallibly obviated by the enclosure of the lamp in a cylinder of metallic gauze. This particular fact seems to establish this general truth, that flame cannot pass through tubes of small diameter—iron network being in fact a series of such tubes. Reflecting upon this, I was led to ask the question, Would not wire-gauze similar to that employed in the construction of the lamp, stretched across the aperture, through which the condensed gases in the reservoir of the oxy-hydrogen blow-pipe rushes into the pipe, confine any explosion which the reflux of the flame might occasion to the pipe itself, and thus completely ensure the safety of the operator, and the apparatus which he employs?

Conceiving the idea involved in the question not to be altogether destitute of plausibility, I was induced to make the following experiments, which, though necessarily rude, from the impossibility of procuring proper apparatus in a country town, seem yet to establish the proposition for the elucidation of which they were instituted.

I employed a tin tube of about six inches long and one inch in diameter, closed at one of its extremities. Having filled it with the gases in their requisite proportion, and put over the open extremity a lid of iron gauze, which had been previously made to adapt to it, I turned up the end which was in the water, and introduced it into a vessel already filled with the same mixture of gases which the tube contained. This I exploded, and immediately plunged the tube into the water. I then took off the wire, raised it from the surface, and applied a taper. No explosion took place; clearly evincing that all had exploded at the same time, and consequently that single gauze had produced no preventive effect.

I then employed double gauze; and proceeding exactly in the same manner as in the first experiment, I found, after having exploded the vessel in which the wired extremity of the tube was immersed, and having turned it down upon the water, that upon taking off the gauze, raising it from the surface, and applying a taper, an explosion was produced—apparently showing that the double gauze had prevented the communication of the flame. I repeated the experiment for a great many times, and in every instance obtained the same result.

I conceived that in the mode of procedure now described this fallacy might occur: while the gauze was put on under the water, might not its apertures be filled with it, and thus prevent the passage of the gases through them? To obviate this, I did not put on the wire till the tube was raised from the water altogether. With this precaution, the result was the same as in the former case.

Yet even here the passage of the gases in the tube, through the gauze, to the gases contained in the exterior vessel was not certain; and thinking that were this circumstance ascertained nothing would be wanting to the certainty of the experiment, I had recourse to this expedient: having taken a tube nine inches long and one inch in diameter, with double gauze inserted in the middle, I filled it completely with the gases, oiled the gauze to prevent the adhesion of the water to its apertures, and closed both its extremities. Having then agitated the tube to make certain the passage of the gases through the gauze, I applied a taper successively to its ends, and found that each of them exploded; again demonstrating the preventive effect of the gauze.

So far have these experiments been successful; and it is not in my power, while in the country, to extend them by their application to the blow-pipe. They seem, however, in some degree, to warrant the success of such an application.

Since the good effect of Sir H. Davy's arrangement depends upon the perfect construction of the cylinder, to render their application to the blow-pipe not only safe, but simple, the gauze might be adjusted to the pipe, and not to the reservoir, to which it might be made to fix with a screw, and in this way be examined every time before commencing the operation.

Kelso, April 24, 1817. G. GRAY.

III. *Further Improvement in the Oxygen and Hydrogen Blow-pipe.*

(To Dr. Thomson.)

DEAR SIR, *April* 18, 1817.

Necessity, ever prompting the ingenious to further inquiry, induced Dr. Clarke, from the suggestion of Dr. Wollaston, to form the fagot of capillary tubes for the passage of the mixed gases to the jet. Struck with the ingenious contrivance, it immediately occurred to me that some time since, having occasion to distil some acetic acid, and not having proper apparatus by me at the time, I made use of a Florence flask, connected with a receiver by a piece of bent cane, and found it answer instead of a tube beyond my expectation, which suggested to me the idea of introducing cane, or any other wood sufficiently porous, instead of the brass capillary tubes, for the passage of the gaseous mixture; or instead of cane, suppose a fagot of very small steel or iron wires made taught by driving in a stronger one, the space between the wires being as so many capillary tubes. I drove a piece of cane 1¼ inch long and one inch diameter into a brass cylinder, in connection with a gaseous blow-pipe, and found the gases pass with the greatest facility. I consider the methods above proposed will obviate the difficulties that may occur in procuring tubes sufficiently small.

I beg leave to propose the following queries:—

1. May not the phenomena exhibited by the gaseous blow-pipe be analogous to that produced by a galvanic battery, the oxygen and hydrogen disengaged at the negative and positive ends being ignited by electricity?

2. Having heard that oxygen gas is often conveyed to a distance in bottles, I think it might be more profitably done by condensing it into a strong metal vessel previously exhausted. By this means a

considerable quantity may be conveyed to a distance. By connecting the vessel with a gasometer, the gas might be let out at pleasure. On this principle rooms at a distance from gas works could be lighted for the evening, by sending an order to any of the gas companies, who should be provided with proper vessels for conveying the gas in a highly condensed state, with a gasometer, stop-cocks, and tubes, for conveying the gas on a very small scale.

I remain, dear Sir, yours, &c.

J. T. Beale.

P.S. Performing some electrical experiments, I conceived spirits of turpentine would insulate. I accordingly rubbed a quantity on the glass support of the prime conductor, and found the insulation apparently as perfect as before. I put a quantity of spirits of turpentine on an insulated brass plate, and electrifying it, the turpentine was driven off a considerable distance in very fine streams. If you think any of these communications not new, or unworthy of a place in your *Annals of Philosophy*, you will reject them accordingly.

IV. *Musical Experiment.*

(To Dr. Thomson.)

SIR, *Red Lion-square, May* 7, 1817.

In your last number of the *Annals of Philosophy* was inserted a wonderful and mysterious experiment on electric attraction and vibration of sounds; but not being possessed of the wonderful abilities of your correspondent, who must certainly have had recourse to some supernatural powers, I was not able to produce the surprising phenomenon said to be the result of his experiment.

With regard to vibration of sound, I have often tried a very pleasing experiment; which, though I dare say it is very well known, yet may be found worthy your notice; namely, that of playing a flute, or any other wind instrument, close to the wires of a piano forte. The vibration of the dulcet notes of the flute on the wires of the piano produces so soft and pleasing music, that it resembles in some measure the Æolian harp; but with this difference, the sound of the flute is so completely intermingled with that of the wires, that it has a great advantage over the Æolian harp. But it is to be observed that this experiment labours under great disadvantage from the low notes of the echo, which is only distinctly audible by applying the ear close to the piano; but should it be brought to perfection by increasing the height of its tone, it would be a very sweet and harmonious accompaniment. Though this experiment is not tinctured with so much of the marvellous as that of your last number, yet should you think it worthy a place in your *Annals*, it may suggest to the mind of some ingenious reader an improvement on it, in which its beauty would be brought to a state of *visible* perfection.

Believe me to be, Sir,

Yours with the most profound respect,

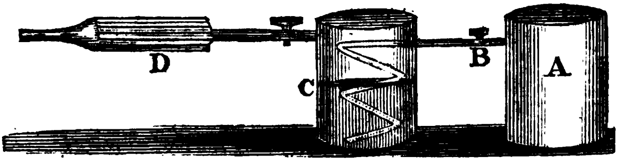
T. L.

V. *Improvement in the Oxygen and Hydrogen Blow-pipe.*

(To Dr. Thomson.)

SIR, *Worcester, May* 18, 1817.

Having observed in your Journal some accounts of explosions taking place, even with Professor Cumming's safety cylinder, from the frequency of the minor explosions forcing back the oil into the condensing cylinder, I take the liberty of suggesting the expediency of placing a pipe of a zigzag form instead of that which commonly communicates between the two cylinders, as in the following sketch:—



A, the condensing cylinder. B, pipe of a zigzag form. C, cylinder with oil. D, Dr. Clarke's fagot of brass tubes.

I am, Sir, yours truly,

FRANCIS SPILSBURY.

VI. *New Mineral Salt.*

Some weeks ago Mr. Heuland presented me with a specimen of a salt which had been brought to this country from Calatayud, in Arragon, by the Spanish Ambassador, and which he informed me was a compound of sulphate of soda and sulphate of magnesia.

The specimen was remarkable for its beauty. The following description will convey some idea of it to the reader:—

Colour snow-white. It was about two inches long, of a fibrous texture, and had quite the aspect of fibrous gypsum. Each fibre, on examination, could be recognized as a four-sided prism, about two inches in length. The prisms were easily separated from each other. The specimen was visibly intersected by 21 divisions crossing the prisms at right angles, so that it had the appearance of being composed of 22 different layers or strata. Lustre shining and silky. Translucent. Soft. Brittle. Very easily frangible. Specific gravity 1·5577. Taste intensely bitter. Not altered by exposure to the air. Very soluble in water.

1. When heated, it dissolves readily in its water of crystallization; and when exposed to a red heat, loses half its weight.

2. Ten grains of it were dissolved in water, and precipitated by muriate of barytes. The sulphate of barytes formed weighed 9·8 grains; indicating 3·32 grains of sulphuric acid.

3. Ten grains were dissolved in water, and a solution of pure soda was mixed with it. The magnesia precipitated being separated by the filter, and heated to redness, weighed 1·62 grains.

From these three experiments it follows that the constituents of this salt are as follows:—

Water	5·00
Sulphuric acid	3·32
Magnesia	1·62
Soda	0·06
	10·00

But 1·62 magnesia require for saturation 3·24 sulphuric acid; and ·06 soda require for saturation ·075 sulphuric acid. Hence the constituents of this salt must be as follows:—

Water	50
Sulphate of magnesia	48·6
Sulphate of soda	1·35
	99·95
Loss	0·05
	100·00

The quantity of sulphate of soda is so small, that probably it is not combined chemically with the Epsom salt. The proportions indicate nearly 42 atoms of sulphate of magnesia to one atom of sulphate of soda. The form of the crystals is that of Epsom salt.

No salt precisely the same with this has been hitherto described by mineralogists. I do not know, indeed, that the salt called by mineralogists *native Epsom salt* has ever been analyzed; but its external characters differ very materially from those of the salt which I have described. The native salt called *reissite* by Karsten differs materially from our salt, both in its characters and composition—two-thirds of it being Glauber's salt, and not quite one-third of it sulphate of magnesia. Besides, it contains a little muriate of magnesia and a little sulphate of lime, both of which are wanting in our salt.

VII. *Death of Dr. Odier.*

Dr. Odier, Professor of Medicine at Geneva, and fellow of various learned societies, died at Geneva on April 14, of an angina pectoris, at the age of 69. His long and very extensive practice, his various works, all of them highly esteemed, and his different courses of lectures, have established his reputation. His death has occasioned the most lively regret. The public loses in him not only a skilful physician, but a zealous citizen, always ready to perform the painful and gratuitous functions to which he was called, and for which he was adapted by his talents, his knowledge, and his uncommon skill. His character and the sweetness of his temper rendered him dear to society. His family and friends are inconsolable for his loss.

VIII. *Remarkable Tree.*

It has generally been observed that when a tree is deprived of its bark it loses the power of vegetating. I have had an opportunity, however, of witnessing two exceptions to this rule. The first in a tree growing a little on the south side of the Meadows at Edinburgh, to the east of the house in which Principal Robertson, the historian, died. The trunk of this tree was deprived of its bark for several feet from the ground upwards, and yet vegetated at least for three years, apparently as well as ever. I do not know whether it still exists, for it is five years since I saw it. I forget what species of tree it was, but rather think it was an elm. The other example is rather more striking. It exists at present in St. James's Park. At that end of the road leading across the Regent's Bridge which terminates at the Birdcage Walk there is a large elm-tree nearly in the middle of the road. The trunk of it is entirely spripped of its bark for at least six feet all around at the lowest part. Last summer it was covered with leaves; but at present only two or three branches on the west side of the tree are in leaf. All the rest of the tree seems dead. These branches, however, may be seen at present covered with leaves. Probably this is the last season that the tree will put forth leaves.

IX. *Morphium.*

This is the name given by M. Sertürner to a substance which, according to him, constitutes the characteristic constituent of opium. From the properties which he has given, it seems entitled to be considered as a new species of *combustible alkali.* It has many points in common with ammonia; but differs from that alkali in being a solid body instead of a gas. It seems to stand in the same relation to ammonia that iodine does to chlorine.

M. Sertürner obtained it in the following manner:—Into an infusion of opium made with water acidulated with acetic acid, pour an excess of ammonia. Morphium immediately precipitates in abundance. It is somewhat coloured by extractive matter; but M. Sertürner says, that if it be agitated with a little alcohol the colouring matter dissolves, and the morphium is left in a state of considerable purity.

It is colourless. It dissolves only sparingly in boiling water; but it is very soluble in alcohol and ether. The solution has a very bitter taste. The morphium may be obtained from it in crystals; the shape of which is a sharp four-sided pyramid, whose base is either a square or a rectangle. Sometimes these pyramids are applied base to base, constituting an octahedron. The solution of morphium gives a brown colour to turmeric paper, and restores the blue colour to litmus paper reddened by vinegar.

It combines readily with the different acids, and forms a new kind of salts, which deserve particular attention.

Subcarbonate of morphium is formed when morphium is placed in contact with carbonic acid gas, or when it is precipitated from its solutions by an alkaline subcarbonate. It is more soluble in water than morphium, and capable of crystallizing. The *carbonate* of morphium crystallizes in short prisms.

Acetate of morphium crystallizes in soft prisms, and is very soluble in water.

Sulphate of morphium crystallizes in the form of twigs and branches of trees, and is likewise very soluble.

Muriate of morphium assumes a plumose appearance. It is much less soluble in water than the other salts of morphium; and when the solution is too far evaporated, it speedily concretes, on cooling, into a shining, silver-white, plumose, saline mass.

Nitrate of morphium crystallizes in prisms, which are grouped together, and appear to issue from a central point.

Meconiate of morphium was not examined; but *submeconiate* of morphium crystallizes in oblique prisms. This is the substance which Derosne extracted from opium, and which he considered as the narcotic principle. It is but sparingly soluble in water.*

Tartrate of morphium crystallizes in prisms, and has a close resemblance to the preceding salts.

Morphium melts in a gentle heat; and in that state has very much the appearance of melted sulphur. On cooling, it again crystallizes. It burns easily; and when heated in close vessels, leaves a solid, resinous, black matter, having a peculiar smell. It combines with sulphur by the assistance of heat; but the combination is speedily destroyed, and sulphureted hydrogen gas evolved.

It acts with great energy on the animal economy. A grain and a half taken at three different times produced such violent symptoms upon three young men of 17 years of age, that Sertürner was alarmed lest the consequences should have been fatal.

Such is an abstract of the properties of morphium as detailed by Sertürner. I shall publish a translation of the whole paper as soon as I can find room for it. Meanwhile, the preceding account will enable my readers to obtain morphium at pleasure, and to investigate its properties.

X. *Safety Lamps for Mines.*

(To Dr. Thomson.)

SIR,

I now send for your inspection, and through the medium of your *Annals* for the inspection of those who take an interest in the security of the laborious miner, two plans of safety lanterns. (Plate LXVII. Figs. 11 and 12.) Unwilling to encroach too much on your pages, I will be very brief in my description.

The large one (Fig. 12) may be styled a double-cased lantern, in which each case has securely fixed its corresponding slips of glass or horn to allow sufficient emission of light. All the parts must be so constructed as to allow no passage for the air but through the after-mentioned openings. The bottom is so fitted as to allow of being taken off for the introduction of the oil lamp, A. Two circular rims, B, C, are made to fit the inner and outer sides of the case. The piece, D, is bent into an obtuse angle (say 110°), both sides of which are cut into very fine parallel passages extending all round for supplying the lamp with air. In the lower edge of the outer case passages are cut; but, instead of being continued all round, are divided into eight or ten equal passages, with the same number and size of intervening spaces uncut. Thus the circumference is divided into 16 or 20 sets of spaces alternately open and shut. The outer rim of the bottom is also similarly divided and cut; so that by turning the bottom round to the extent of one of the spaces, you either completely shut or completely open the passages, The three conical tops (E, 1, 2, 3,) are cut into fine parallel vertical slits. The air passages may be from $\frac{3}{10}$ to $\frac{4}{10}$ of an inch in length, and must not exceed $\frac{1}{70}$ or $\frac{1}{80}$ part of an inch in width: indeed, the finer the openings, the greater security is afforded. They should be cut with a sharp chisel upon a leaden block, and the sharp edge or bore should be made to stand outwards, in opposition to the current of air rushing inwards. The air must pass through three of these gratings before it can reach the flame.

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* Meconic acid (from *μηκων*, a poppy) is a peculiar acid which Sertürner has detected in opium.

The small drawing (Fig. 11) represents the lantern proposed in my last letter to be fixed on the jets. It is simply an Argand's glass chimney covered with tin plate, and so cut as to allow free diffusion of light all around. The top and bottom are each composed of one horizontal and one upright air grating.

I learn from Mr. Wilson, a young medical gentleman belonging to the navy, that canvas pipes are there actually applied to purposes similar to what I suggested in my last letter.

Canvas tubes intended for ventilation may be constructed in the following manner:—Provide a wooden mould of the form, length, and size, of the intended tubes; also rings of wire for each end, and a slip as long as the tubes of plate or hoop iron, into which two or more staples or hooks must be riveted for suspending each tube by itself. The canvas is then to be cut into pieces, so long and so wide that about three inches shall overlap the mould. Then each piece is to be coated with strong paint, and applied round the mould. The iron slip is now to be inserted into the double part, where it is secured by a sowing on each side. The rings are also to be secured by sowing; and, lastly, the whole may be covered over with an outside coating of paint.

It must be obvious to you that in the engraving, Plate LXV. Fig. 5, the two arrows intended to represent the current of air from the unshaded lateral tubes into the main tube are wrongly directed.

I remain, Sir, with great respect,

Your most obedient servant,

Glasgow, April 24, 1817. HUGH WALLACE.

XI. *Experiments on Pendulum Vibrations at different Latitudes.*

The long talked of experiments and observations in reference to the figure of the earth, and the lengths and vibrations of pendulums in different latitudes, are now in progress. Colonel Mudge, the conductor of the Trigonometrical Survey, and M. Biot, of the French Institute of the Paris Academy, have gone together to Edinburgh. M. Biot is now making the pendulum experiments at Edinburgh; while Colonel Mudge and Captain Colby are measuring a base of verification near Aberdeen. The operations at Edinburgh and Aberdeen are expected to terminate about the middle of June; when the party will be joined at Aberdeen by Dr. Gregory, of the Royal Military Academy; and the whole will proceed to the Orkneys, as well for the purpose of making the requisite astronomical observations, as for that of conducting the pendulum experiments, both with M. Biot's apparatus, and with the astronomical clock taken out by Colonel Mudge.

XII. *Mr. John Stevenson, Oculist, &c.*

In consideration of the personal benefits received from the professional talents of John Stevenson, Esq. of Great Russel-street, Bloomsbury-square, his Royal Highness the Duke of York has been graciously pleased to appoint him his Surgeon-Oculist and Aurist.

ARTICLE XII.

Scientific Books in hand, or in the Press.

Dr. W. Philip has in the press, nearly ready for publication, an Experimental Inquiry into the Laws of the Vital Functions, with Observations on the Nature and Treatment of Internal Diseases, in part republished, by permission of the President of the Royal Society, from the Philosophical Transactions, with the Report of the National Institute of France on the Experiments of M. le Gallois, and Observations on that Report.

Mr. George Ogg, of Plymouth, has just published a Lecture which was read to the Plymouth Institution on the Prevention and Cure of Dry Rot in Ships of War.

Dr. Spurzheim has just published, in 8vo. an Inquiry into the Diseased Manifestations of the Mind, or Insanity, illustrated with four Plates. He is also preparing a new work, being a Series of Essays on the Forms of Heads, in relation to the different Characters, Individual and National, and on the Principles of Expression.

Mr. Nicholas is about to publish the Journal of a Voyage to New Zealand, made in company with the Rev. Samuel Marsden.

Dr. Duncan, jun. of Edinburgh, has nearly completed the New Edition of the Edinburgh Practice of Physic.

The Second Volume of Kirby's and Spence's Introduction to Entomology is nearly ready for publication.

ARTICLE XIII.

METEOROLOGICAL TABLE.

1817.	Wind.	Barometer.			Thermometer.			Hygr. at 9 a. m.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
4th Mo.										
April 8	Var.	30·20	29·89	30·045	58	34	46·0	59	1	☾
9	N E	29·97	29·89	29·930	46	28	37·0	46	—	
10	N	30·23	29·97	30·100	40	25	32·5	50	—	
11	N W	30·23	30·02	30·125	46	29	37·5	53		
12	N W	30·01	29·96	29·985	49	39	40·5	63		
13	N W	30 04	30·00	30·020	55	38	46·5	53	6	
14	N W	30·00	29·93	29·965	60	42	51·0	55	—	
15	N W	29·72	29·67	29·695	61	41	51·0	50	—	
16	N	30·11	29·72	29 915	48	32	40·0	52	2	●
17	N	30·30	30·11	30·205	42	34	38·0	43		
18	N	30·37	30·30	30·335	53	26	39·5	50		
19	N E	30·32	30·30	30·310	55	40	47·5	42		
20	N E	30·34	30·33	30·335	55	34	44·5	50		
21	N E	30·34	30·27	30·305	59	32	45·5	46		
22	S E	30·27	30·17	30·220	57	29	43·0	60		
23	N E	30·20	30·17	30·185	50	27	38·5	59	3	
24	N E	30·20	30·14	30·170	52	35	43·5	52	2	☽
25	N E	30·14	30·12	30·130	44	36	40·0	46		
26	N E	29·93	29·87	29·900	49	40	44·5	44		
27	Var.	30·09	29·93	30·010	50	32	41·0	40		
28	W	30·09	29·91	30·000	58	43	50·5	45		
29	N W	29·91	29·70	29·805	48	37	42·5	47	—	
30	N E	29·81	29·69	29·750	50	39	44·5	55	·10	
5th Mo.										
May 1	N E	29·93	29·81	29·870	48	34	41·0	50		○
2	N W	29·93	29·84	29·885	56	30	43·0	42		
3	W	29·77	29·72	29·745	60	45	52·5	45	—	
4	N W	30·01	29·77	29·890	60	32	46·0	41		
5	S W	30·06	29·95	30·005	64	35	49·5	48		
6	N	30·16	30·06	30·110	64	36	50·0	34		
7	S E	30·06	29·77	29·915	60	37	48·5	50	4	
		30·37	29·67	30·028	64	25	43·85	49	0·28	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Fourth Month.—8. The wind was for some time at SW: rain in the night. 9. Cloudy, a. m.: a shower of driven granular snow in the night. 10. *Cumulostrati* and *Nimbi,* giving small quantities of snow. 11. *Cumulostratus:* windy. 12. Mostly overcast: very light rain at intervals. 13. Small rain, a. m.: fair, p. m. 14. A little light rain. 15. Fair: large plumose *Cirri* above *Cumuli.* 16. a. m. A strong gale from NW and N, with a shower and hail: rainbow: fair day after. 17. *Cumulostratus:* dark sky: windy. 18. *Cumulostratus:* the wind veers to NE and NW: calmer day. 19. The hygrometer noted at 10: *Cumulostrati* prevailed, surmounted by the lighter modifications: windy: the part of the moon's disc in shade distinctly visible, and the light crescent very conspicuous in the evening: a small meteor passed to the NE. 20. a. m. Windy, not steady to NE: *Cumulostrati.* 21. a. m. *Cirri* pointing westward, with *Cumuli* beneath: afterwards an arrangement of this cloud in regular parallel streamers from NW to SE, which became red at sun-set. 22. With the SE wind this morning the *swallows* appeared, but few in number, and flying feebly: a serene evening, after *Cumulus* and *Cumulostratus.* 23. Hoar frost early: cloudy: windy: a shower from NE, p. m.: clear evening: the hygrometer to-day receded to 32°, and the superior part of the clouds, after the rain, presented a configuration like the *pores of sponge,* which I have not observed before for some years. 24. *Cumulostratus*: windy: a shower at night. 25. a. m. Overcast: windy: *Cumulostratus.* 26. The same, the breeze growing stronger. 27, 28. Chiefly overcast with *Cumulostratus* and large *Cirrocumulus.* 29. The same, with *Cirrostratus:* a slight shower by night. 30. A moderate gale at NE, with showers and much cloud: *Nimbi:* a little hail.

Fifth Month.—1. Cloudy: windy. 3. A slight shower in the night. 5. The hygrometer receded to 32°. 6. The wind went from N to E. 7. Wind SE: a breeze: very clear all day, and a full orange twilight: by six, a. m. the 8th, it was however SW, with a slight shower.

RESULTS.

Winds almost uniformly northerly, and moderate in force.

Barometer:	Greatest height	30·37 inches
	Least	29·67
	Mean of the period	30·028
Thermometer:	Greatest height	64°
	Least	25
	Mean of the period	43·85

Mean of the hygrometer, 49°. Rain, 0·28 in.

Vegetation has been peculiarly slow during this period.

Tottenham, L. HOWARD.
Fifth Month, 10, 1817.

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